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THE JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

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EDITORIALS

THE NEW PROGRAM OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

We note with satisfaction that for the coming meeting of the American Psychological Association "in view of the persistent criticism of the overburdened programs, and in order to further promote discussion and to diminish the conflict that arises from the holding of simultaneous sessions, the Program Committee . . . will limit the number of papers on any scientific program to five, keeping the number of such programs as at present at approximately nine."

In place of the usual large number of formal papers the committee will increase the number of round table discussions and has proposed thirty-six different subjects for such discussion. The list, which we reproduce, covers, as will be seen, nearly every topic of present interest:

Mind and Body
Consciousness and Behavior
Introspection as Method
Statistical Methods
The Correlation Methods
Psychophysical Measurement
Vision
Audition
Taste and Smell
Space Perception
Seen Movement
Feeling and Emotion
Psychogalvanic Reflex
Facial and Postural Expression of Emotion
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Personality
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Motor Learning
Nature of Attention
Thought
Language
Esthetics
Sleep, Dreams, Hypnosis
Abnormal Psychology
Mental Tests
Educational Tests
Clinical Psychology

Relations of Nature and Nurture	Animal Behavior
Individual Differences	Animal Ideation
Methods of Learning	Motivation
Childhood and Adolescence	Requirements of Degree of Doctor of Philosophy

"The Program Committee hopes to print the names of all members and associates in the program who have indicated their interest in any special round table discussion in which they expect to participate."

We are glad to endorse this decision of the committee. We believe it to be a long step in the right direction. Round tables, where problems can be informally discussed and those specially interested and qualified to discuss them can lay their minds side by side, are the order of the day. It is the method that has been adopted in various fields of interest—politics, economics, science, foreign policies, international relations, etc. At a round table differences of opinion and the relative values of methods can be ironed out, conflicting data compared, points of agreement and disagreement clarified, and confusion of interpretation cleared up much more satisfactorily than by formal papers and discussions at general meetings.

We hope that all members and associates will coöperate to make this program as successful as it promises and ought to be.

A MEDICAL OFFICER IN THE PRESIDENT'S CABINET!

> Be it Resolved: It is the sentiment of the American Medical Editors' Association that there should be a Medical Officer in the President's Cabinet at Washington. That such an office be created and that the interest of the medical profession should be aroused, and that editorials be written to appear in the medical journals of this country, for the purpose of eventually accomplishing this result.

The above remarkable resolution—remarkable, we mean, for its style of English composition—was sent to us by the American Medical Editors' Association. The same communication stated that it was adopted, be it noted, by the Executive Council of the Association (not by the Association itself), and expressed a conviction that we "will wish to publish it in an early issue" of this Journal "with an editorial setting forth the need of a medical

officer in the President's Cabinet *to look after the interests of the medical profession* and to represent the public health of the nation" (italics ours).

We are not conscious of any such wish and we hope we have none that is subconscious. Allowing ourselves after reading the resolution to fall into a state of abstraction and to discover by the method of free association whether there might be such a wish in Freud's unconscious, the first memory that came to consciousness was that of the famous resolution of the "three tailors of London" beginning "we the people of England". And then there came the thought of "the butcher, the baker, the candlestick maker" demanding a Cabinet member to look after their interests. This was followed by visions of osteopaths, chiropractors, architects, artists, and undertakers clamoring for representation in the President's Cabinet.

As to the need of a Cabinet member "to represent the health of the nation", we wonder if the Executive Council of the A.M.E.A. have ever heard of the Constitution of the United States, wherein "the powers not delegated to the United States . . . are reserved to the states respectively or to the people"; surely the public health is one of these rights. Furthermore, although there are certain limited functions involving public health, such as quarantine and scientific research, that can be and are carried out by the Federal Government, these are properly and adequately administered by a bureau of one of the departments. As a matter of public policy and practicality it is recognized by common consent that care for the public health is a function of and best carried out by local governments, state and municipal. The proposal to have a Cabinet officer of health is involved in the great public policy of decentralization versus centralization of all government in Washington. The growing habit of groups interested in special subjects rushing to Washington to have the Federal Government take over activities that should be performed by the state is fast becoming a grave danger. We have seen examples of this in well-meaning philanthropists interested in child labor, maternity, etc.

As to arousing the medical profession by editorials as we are urged to do by the resolution quoted above, we think the profession is well able to look after itself. We are not aware that the American Medical Association has adopted any such foolish program. If it has, it had better reconsider before being laughed out of court.

REACTION TIME AND ALLIED MEASURES UNDER HYPNOSIS: REPORT OF A CASE

F. L. WELLS

BOSTON PSYCHOPATHIC HOSPITAL

I

THE original purpose of this note is to record certain observations on the time of psychophysiological processes under hypnosis, made some five years ago with a case affording special opportunity for such measurements. The case presents sufficient features of interest from other points of view to warrant somewhat more detailed account than would be strictly pertinent to the experimental work itself.

The patient is a native American of twenty-seven at the time of the observations, brought under the complaint of having made an attack with a knife upon his sister. In the family history there are no special features. All other members, of whom there are several, have assimilated the elementary school education, and adjusted themselves reasonably well, though at a rather subaverage social level.

Physically the patient is tall, poorly developed and on admission poorly nourished, long features, fingers and toes extremely long, chest narrow and "barrel"-shaped. Some degree of kyphosis is present. The physique would rate typically among the asthenic in the recent classifications.

With regard to intellectual functions, his school progress was a little slow, reaching 7th grade at fifteen. He continued study at night high schools, and was said to have come within a year of graduating. In hospital, however, he reached an I.Q. Stanford of no more than 79, in the presence of intense effort, but difficulty in "concentration". For some six years since attending school he has held a great many short time positions, always of a type involving very little responsibility, and for which there was no keen competition. He was a satisfactory low grade employe, working faithfully and conscientiously at his job, but easily enticed to another by superficial attractions.

Socially, he was in childhood, at least after ten years of age, distinctly seclusive. His underactivity is characterized in his father's saying that his brother would wear out five pairs of shoes to the patient's one. His social adjustment in school was much

better to girls than to boys, this not on a sex basis, but because of his having more interests in common with them. This has continued to be true in the commercial establishments where he has worked.

He uses no tobacco, alcohol, drugs or tea; on the other hand, consumes 12 or more cups of coffee per day. There has been a compulsion to touch twice objects that attract his notice; once he ran back from school to touch a building he had missed on the way there. This compulsion he superficially rationalizes in connection with his masturbation; he may perish during one of these episodes, and never get a chance to touch the object again. His gait on the street is mincing, and he sometimes feared to walk out for the attention it attracted. His ablutions are only normally frequent, but he spends excessively long times at them. Most of his life he was very finicky about his clothes, but about a year before admission swung over to the opposite extreme. A comparable change since also came about in his attitude towards his physique. He long entertained the ambition to become a dancer or some other artistic figure; in connection with which, he desired to avoid a masculine type of body, and to keep his muscles slender. A few weeks before admission he was the victim of some physical violence in a street attack, and upon this he developed much interest in acquiring musculature, learning boxing, reading physical culture magazines.

A characteristic suggestibility and emotional sensitiveness appear in an episode when as a child in a school performance he had a dance partner who was a colored girl. He was told that he would become black if he touched her, and when on the occasion of the performance he had to do this, he ran home, hid in a barrel, and would not look in a mirror, in spite of all reassurances, for several days. On another occasion when told by the school principal he had not passed his school grade, he became "hysterical", wept violently, and had to be taken home by a school official. At the hospital, he fainted when his blood was taken for a Wassermann. When, his case being presented at staff meeting, he was asked in general about what he sought from life, he poured out a verbal dream picture of leading a great symphony; of painting people with golden crowns, angels with flowing hair and beautiful robes; of the "coming out parties" of beautiful girls and working himself up to a greater pitch of emotion than he could tolerate, burst dramatically into tears and rushed from the room.

There was intense attachment to the mother, who gave in to his idiosyncrasies, would do things that he refused, leaving him

idle. Since her death he has spent whole nights at her grave, weeping. When his mother died he got a bottle of some medicine that had been used by her, and sought out a corner in a church to commit suicide. "While there, he saw a colored man who seemed to be extremely happy in prayer, and this (he said) was such an inspiration to him, namely, that if a colored man could be happy in this life, why then he too, could be happy", that as a result he abandoned his intention. Subsequently he constructed at home an elaborate device whereby the opening of the patient's bedroom door by one of his family would release a suspended bucksaw, which would fall on the patient's neck and kill him. One reason for the failure of this attempt was that, the family thinking he was out, no one opened the door. The attack on the sister, which led to his admission, was of a similar level. He merely placed the knife against the body, without pressing it. His sister was sure he would do nothing, and smiled calmly at him. The motivation of his attack on the sister is unclear, but she was the center of very great affects for the patient, having apparently replaced the mother in this respect. Tears would come into his eyes when he talked of her. The patient had been much upset by her friendship with another girl, of whom he did not approve, and he had made some indiscreet attempts to separate them.

Nothing is recorded of the sex life of the patient in early years. At eleven years he had the sex act with a girl of sixteen, in which she was presumably the aggressor. Masturbation began upon this episode, and grew to frequency of several times a day, though this perhaps not so much through gonadal hyperactivity as "irritable weakness", inability to accumulate any degree of tension of this kind (see below). He would have emissions while dancing. There was much conflict over the autoerotism, and he rationalized through it his preference for the society of "small boys of fourteen or sixteen", as he was not fit to associate with men. In other ways his sex concepts and information are at an infantile level. Questions about marriage he answers as a young child, knowing nothing of sex urge, might do. On the other hand, his masturbation fancies contain a marked sadistic element, directed at the girl friend of his sister's of whom he is jealous; during these episodes he swears and curses violently. Semen is a poisonous substance; he does not masturbate with his hands because it might get in his mouth and injure him. For the same reason he does not wash his genitals. Certain foods are disliked because they suggest to him the appearance of semen (cf. with these ideas his desire for poison with which to commit suicide).

Recent experiences are reported which approach very nearly, if they do not quite reach, the level of hallucinations, and other dissociative phenomena. He believes in a guardian angel (Baucis) a blond motherly woman who watches over him, and her associate (Philemon), a stern man with a short beard. He actually sees them together and apart in a setting of a "far away land of purple and yellow colorings". He hears the voice of (Baucis) and at times sees her form, she guides and advises him, will some day take him to her "land of Philemon", a haven of rest and happiness. Mingled with these are vague scenes of the murder and destruction of his enemies.

Quoting the history, the patient says at times he feels as if there were two parts to himself. One argues with the other. He calls one his stronger and meaner self, that is always persuading him to do mean and antagonistic things, while his weaker self argues back against this, standing for good and proper actions. These he sometimes hears as actual voices.

At this first admission the patient remained in the hospital some eight months, during which time he made slight gain in stability, but was little able to occupy himself in any constructive way. He was then transferred to another hospital where he seems to have further improved, being able to carry on simple routine work assigned to him. After five months he was considered well enough for discharge. He was, however, consistently unable to find employment outside nor were the circumstances of his family such that they could help significantly in his adjustment. Twice since, he has been readmitted to this hospital, being unable to exist outside, and in this shelter he has apparently been able to do simple work, at a level about sufficient to meet the cost of maintaining him. At his first admission, the dissociative symptoms led to a classification of dementia praecox but with the subsidence of these the subsequent diagnosis has been without psychosis. There has been no direct contact with the patient for upwards of two years. Through other sources it is learned that his general adjustment level continued as before. He could apparently get no work except through charitable agencies, and did not hold any job long. It is not clear how he managed to exist at all, but a few days previous to this writing he chanced to be seen on the street by a physician formerly of this hospital, in a most wretched condition. He attempted to avoid the physician, but the latter took him to his own home, generally outdid the prodigal's father towards him (the patient is Christian, the physician Jewish), and found him work. In view of the previous record, however, one must be very

optimistic to regard the adjustment as having other than a temporary character.

This case is a very pertinent one to consider in connection with a recent formulation of L. K. Frank's.(1) In these terms, the patient is characterized to a remarkable extent by inability to accumulate the tensions necessary for an effective disposal of energy upon the tasks of life. As has been seen, the least sex tension is dissipated in masturbation, other ambitions in daydreaming, at times hallucinosis. This is in part what gives his personality its infantile aspect, for it is also characteristic of the child that he cannot accumulate the tensions of the adult. Goddard has emphasized a similar point in connection with the feebleminded, for example, where the normal child would choose a whole holiday next week, the inferior will prefer a half-holiday this afternoon. The deferred satisfaction involves a capacity for accumulating tensions, which they do not possess.[1] A dream-symbol for the patient's mental mechanism would be a steam boiler with no weight on the safety valve. There is a fire under the boiler, not an extremely hot fire perhaps, but still enough to make steam for an engine of average power if it could build up the tension. That is, we do not find in this patient an apathy, but a dissipation of energy at imaginal, autoerotic and visceral-thalamic (emotional) levels; the last named producing at times a picture of marked distress. The ensemble is one suggestive of a mother fixation, and previous paragraphs cite evidence that this existed. It is also recognized that fixations of this sort limit capacity for dealings with one's fellows. Again, a person lacking, as is the patient, in the tensions whose adjustment demands personal contacts, seeks them less, and develops thereby less aptitude in meeting them. As Frank points out, personal encounters are in some sort a medium to practically all normal organic consummations in modern life. He distinguishes a personality type to whom these encounters are facile, and who thus has the most energy to devote to the organic, fundamental consummations (objective type); and one which, through difficulty with personal contacts has his energies absorbed by them, and has less energy for the organic consummations (status type). The present case would be a good illustration of the submissive status type as Frank formulates it.

[1] *Cf.* James, *Principles*, I, p. 23; though not all the illustrations would serve: "The tramp who lives from hour to hour; the bohemian whose engagements are from day to day; the bachelor who builds but for a single life; the father who acts for another generation; the patriot who thinks of a whole community and of many generations, and finally the philosopher and saint whose cares are for humanity and eternity," etc. Also pp. 314–315.

It is quite consistent with the psychogenic background so far as understood, though only a fragmentary understanding can be hoped for. As with other appeals to psychogenesis, the question remains of what made him so responsive to the sensitizing experiences, and this answer may have to be put in organic terms.

From the social standpoint, a personality such as this is difficult to assimilate into any normal cultural organization, of whatever level. At the same time the patient accomplishes much more towards self-maintenance (at skilled labor and clerical levels) than many to whom modern civilization affords special institutional shelter. With adequate intellectual background, the richness of his emotional life would offer a series of values given higher recognition in our culture. As it is, there is a note almost worthy of A. E. Housman in the following lines which the clinical history attributes to the patient:

> Across the fields of yesterday
> He sometimes comes to me,
> A little lad just back from play
> The lad I used to be,
> And yet he smiles so wistfully
> Once he has crept within,
> I wonder if he hopes to see
> The man I might have been.

Many reports of hypnotic phenomena describe increased functional efficiencies during this state, and it was desired to see how far these would be produced with the present subject. A major issue raised was a criterion of the hypnotic condition, its presence or depth, and this it was not possible to meet wholly. There is no attempt here to go beyond a comparison of (1) a "waking" state, *i.e.*, such degree of mental integration as this none too well organized subject normally possessed and (2) a "hypnotic" state, characterized by the simpler overt responses associated with the name of hypnosis, *e.g.*, suggested "catalepsy", inability to speak, open eyes, and the like. The patient was capable of much more dramatic dissociations. The writer was present when on one occasion he reproduced the classical phenomenon of simultaneously carrying on an oral conversation, and writing the answers to another series of questions, whispered in his ear.[2]

The experiments were of several different kinds, the description of whose techniques is for the most part available elsewhere. For the reaction time experiments, the set-up was that utilized in previous studies of vocal and manual reactions (2) and of visual

[2] This observation was conducted by Dr. W. S. Taylor, now of Smith College, who also observed this case.

affective processes (3). The substitution test is that originated by Woodworth (4) and further standardized by Pintner and Paterson (5). The "achievement capacity" test is that of G. G. Fernald (6). The memory test items are from a test described by the writer in connection with other methods of clinical psychometrics (7). For the tapping test, the apparatus and general procedure were also as in the writer's previous studies of this function (8).

Summary of the experiments devoted to simple sound reactions is as follows:

Date	No. Obs.	Av. Time	m. v.	Conditions and other remarks.
8–12–22	14	.232″	15	normal waking
	13	.226	19	waking, increased suggestion for speed
		(Observations with substitution test, see below)		
	27	.221	17	hypnosis, suggestion for maximal speed
		(Observations with substitution test, see below)		
	27	.234	20	normal waking
8–16–22	7	.231	27	normal waking
	8	.227	22	waking, more speed suggested
	15	.220	18	waking, special suggestions discontinued
	15	.270	24	waking, maximal suggestion for speed
8–23–22	(These observations preceded by tapping and tremor tests under hypnosis, see below)			
	7	.239	22	waking, special suggestion before each reaction
	8	.260	18	waking, special suggestions not given
	6	.281	42	hypnosis, suggestion for maximal speed given before each reaction
	8	.262	21	hypnosis, special suggestions not given. At close had eyes shut, could not open on suggestion thereof
	7	.260	11	waking, suggestion for speed at each reaction
	8	.296	37	special suggestions discontinued. At close, shut eyes on suggestion, and unable to open till so suggested, although "technically" awake

The following November a series of more complex reactions was studied, each experiment consisting of simple reactions to sound stimulus, choice reactions in which the right hand was lifted to a white stimulus and the left hand to a black, reactions in which the subject read simple words, a series in which a simple reaction was made by speaking a word, and a series in which simple opposites were named. The voice key used was a "Radio Rex" attachment whose properties in this connection have been elsewhere described (9). In summaries, the various days' experiments on each type of reaction are grouped together, for convenience in comparison. The first, second and fourth series are "normal waking", the third (in italics) is under hypnosis. In this series

the suggestion of special ease and quickness was given, but at the beginning only. Otherwise there was nothing different from the ordinary conditions of suggestion. Between each series of reactions the typical responses are opening and closing the eyes, of movement and immobility of limbs, were elicited. These experiments are summarized as follows:

Date	No. Obs.	Av.	m.v.
(Simple hand reaction to sound)			
11-11-22	10	.269″	38
11-11-22a	10	.297	31
11-13-22	10	.261	45
11-14-22	10	.273	40
(Simple reaction to sound with vocal response)			
11-11-22	5	.534	19
11-11-22a	10	.598	54
11-13-22	10	.616	47
11-14-22	10	.567	25
(Choice reactions to black and white)			
11-11-22	6	.582	58
11-11-22a	10	.596	113
11-13-22	10	.556	50
11-14-22	10	.531	56
(Reading simple words)			
11-11-22	7	1.242	89
11-11-22a	10	1.032	88
11-13-22	10	.988	119
11-14-22	10	.885	48
(Naming simple opposites)			
11-11-22	10	1.371	157
11-11-22a	10	1.231	123
11-13-22	10	1.248	183
11-14-22	10	1.064	72

On December 2 of the same year, a comparison was made under normal waking conditions of choice reactions to black and white (.692″), to true and false sentences (2.260″), to correct and incorrect sums (1.737″), agreeable and disagreeable faces (1.735″). The average time is that quoted in parentheses after each. There are ten observations in each series. The times are longer than averages previously observed (10), though not longer than might be expected in a person showing his intelligence test results. The probability is that both of these results are conditioned not by subaverage intellect in the ordinary sense, so much as by disor-

ganization of mental processes; cf. the "difficulty of concentration" noted by the examiner in the intelligence test. There were five "false" reactions in the experimental series.

The substitution test as devised by Woodworth offers convenient illustration of the acquisition of new memories, and of interference effects through a change of the "key". Attempt was made to see how the normal process of learning this test was modified in the presence of hypnotic suggestion. The first observations are summarized:

Date	Time, 1st half of blank	Time, 2d half of blank	Remarks
8-10-22	89 (1)	76	Normal waking standard key
8-10-22	55	47	Hypnosis, suggestion of heightened efficiency

The course of the improvement (as given in the times of the individual lines) is not that of the practice curve, and suggests a real heightening of speed subsequent to and consequent on the hypnotic suggestion.

The following day, under hypnosis, the key was not shown but suggestions were given of reënforced memory thereof, and of maximal test efficiency. The times of first and second halves were respectively 47 and 39 seconds. The initial line was done in 7 seconds, the quickest line on the previous day having been 9 seconds. The almost invariable lengthening of the second line (here 12 seconds) cannot be attributed to looking at the key, as it was not shown.

Immediately afterwards, all memories of the sheet were suggested lost, it must be learned again *de novo*. The strength of this suggestion would be given in the measured degree of its operation (cf. paper by Hull and Ludden before the American Psychological Association at Philadelphia, 1926). The time of the first half is 45 seconds, the initial line 9 seconds. It was apparently inoperative; but when the suggestion was reintroduced at the 6th line, the remaining 4 lines gave times substantially equal to the last four lines of the initial trial, the day before.

Thereupon the key was changed,[3] and the blank presented with suggested loss of all previous memories thereof, but the new key to be learned with maximal efficiency. The first and second halves of this record total 84 and 87 seconds. As this is slightly longer

[3] These changes of key were effected by cutting from a test blank, successions of the five geometrical forms in orders wholly different from the original one, and numbering each of these orders, 1, 2, 3, 4, 5.

than the initial record, it would appear that interference was present in sufficient degree to outweigh the suggestion of efficient relearning. Another trial under the same conditions showed a total time of 144 seconds, as against 102 seconds for the next to the initial trial, the day before. Then a restoration of memory for the original key was suggested. The halves were 57 and 40 seconds respectively, further corroborating the evidence of interference above, and a minor impression made by the new key. A suggested forgetting at the last line was quite without effect, its time being 7 seconds.

The following day the observations were resumed, beginning with the original key, normal waking, and with special care to present the same degree of suggested efficiency, for both hypnotic and waking conditions. The first series, with the original key (not shown), yielded a total time of 99 seconds. Then with the previously used change, key shown, two waking trials yielded total times of 157 and 129 seconds. Three trials under hypnosis timed 115, 122 and 110 seconds. But there is evidence from the times of the individual lines that practice effect in these records is long continued, and this slight improvement could not be ascribed with any certainty to the hypnosis. Indeed, a final waking record yielded a still shorter total time, 108 seconds.

Two days later, a new key was tried with 3 repetitions under hypnosis, the successive times being 161, 148, 140 seconds respectively. It was apparently harder to learn than the first change, as this had been harder than the original. There was no response to suggested forgetting at the second half of the fourth line, its total time being 131 seconds—this in a subject who could be "put to sleep" with a wave of the hand. A waking trial at the close yielded a total time of 95 seconds. Under similar conditions of suggested efficiency the hypnotic records were inferior.

Irregular results were obtained with certain items of the standard "memory" test tried August 10, 1922. No effect on memory span was traced; it was low, 5 digits forward. In the recognition memory item (post cards) the chief effect of the hypnotic suggestion was to increase the promptness of the responses. The waking score was adult average, under hypnosis one-third less. The effects in the "town-state" tests were more striking. The average waking time for naming the state where a well-known city is located was over 2 seconds; under hypnosis it was never over one second. The "hard town-state" items, in which unfamiliar associations of this kind must be established, were apparently much facilitated under hypnosis.

A Marey sphygmograph was arranged to record hand tremors and the effect of suggested steadiness was observed under waking and hypnotic conditions. There were no consistent differences.

The tapping tests are summarized, according to the number of taps in each successive 5 second interval, as follows:

Date	No. of Taps per 5″ interval	Remarks
8-23-22	29 28 28 26 28 28 28	Waking, normal suggestion
	31 30 29 31 30 29 30 30 30	At end said process went very easy, no fatigue felt, in contrast to first time, when he had "wanted to stop"
	33 33 32 32 29 28 28	Waking, maximal suggestion. Appearance of fatigue at 22″, anti-fatigue suggestion renewed; reports at end no fatigue felt, but seemed to "go smoother" when asleep
8-24-22	35 34 33 31 30 30 28 27	Waking, maximal suggestion. No fatigue felt, but did not seem as fast as yesterday
	33 30 31 29 28 30 30 27	Hypnosis, maximal suggestion. Felt no fatigue, seemed as fast at end as beginning
	32 32 30 29 28 28 28 28	Waking, normal suggestion. Some fatigue, spontaneously compares with hypnosis record, when no fatigue felt

Apparently more effort is put forth upon maximal suggestion under waking than under hypnotic conditions. It is unfortunate that no hypnotic record was taken with "normal suggestion". Under the observed conditions the "fatigue" loss in rate is seen to be less under hypnosis, and the reported absence of fatigue sensations is noteworthy. The question remains at what level they are denied, whether unfelt or because told that they will be so.

The following are notes on the "achievement capacity" test of G. G. Fernald, in which the subject stands as long as he can on his toes, on a mechanism with a pointer to show him how well he is keeping himself up. The experiment under hypnosis follows 12 days after the waking one and was given under suggestions of maximal endurance and immunity to fatigue.

Waking

0	Experiment begun
3′ 30″	"I can't do it much longer"
6′	"I'm tired but I won't give in" (there speaks the status-type!)
8′	Perceptible failure
8′ 30″	Slowly settles backwards from apparatus, is supported

Hypnosis, response to arm and eye tests thereof in chair and when standing before apparatus.

0	Experiment begun
1′ 45″	Some muscular fatigue reported
2′ 45″	Considerable let-down in tremor

3' 45"	Let-down in tremor continued
4' 55"	Rises to limit on continued suggestion and holds it
5' 45"	Says all fatigue gone, stands steadier
6' 45"	Still at maximum height, more tremor
7' 45"	Less tremor, says no fatigue
8' 45"	Reports slight fatigue, doubtful if able to carry on
9' 30"	Marked increase of tremor, loses balance and settles as in waking experiment

There is not much reason to believe that the latter result would have been different if the suggestions had been given to the patient without any hypnotic preliminaries.

With such work as that of Nicholson (11) and more recently of Hull, the hypnotic phenomena are emerging from a semi-apocryphal status into a field of objective and quantitative study. But there is hardly more treacherous ground in experimental psychology. A demand for multiplicity of controls will assert itself strongly to the strict observer. It appears from this case that so far as functional efficiency is concerned the force of suggestion varies greatly with different functions, as was pointed out for normal conditions by Scott (12). Attention must also be paid to equalizing, at least objectively, the force of special suggestions in the various states where effects of the hypnotic process are to be compared. Other problems concern the criteria of the hypnotic state and possible change and fluctuation during the periods under experimental study.

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THE DEVELOPMENT OF THE CONCEPT OF THE SUPER-EGO *

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THIS is the first occasion on which I have had the honor of addressing the French Psycho-Analytical Society, but I recall with pleasure that just two years ago I had the opportunity of speaking before the informal circle out of which the present society has since developed. I wish to offer you my cordial congratulations on the progress your active group has made in this time, in the work of the society itself, in the busy therapeutic labors of the individual members, and in the enterprise you are showing in the publishing field, all of which is of the happiest augury for the future.

If any of those at the start of this brilliant career, about to conquer a new country for psychoanalysis, are willing to listen to a word of advice from one who unfortunately has now more than twenty years of psychoanalytic experience behind him, it would be best crystallized in one phrase: Never underestimate the strength of the unconscious resistances you will meet, both in your patients and in the outer world. These resistances can be withdrawn, concealed, evaded, in a myriad ways, but one is continually making the discovery, to one's chagrin and cost, that one has once more made the mistake of optimistically underestimating their strength and endurance; the opposite mistake I have never yet seen committed.

The theme of my paper to-day is concerned with one of the most interesting aspects of these resistances, though it is by no means the only one. I may say that I have chosen this theme not only because of its intrinsic importance and current interest, but also because it provides the opportunity of emphasizing some considerations of a general order. For the subject of the super-ego affords an interesting bridge between the older and the newer conceptions of psychoanalysis, and so enables us to attain a clear idea of the relations between the two.

If one were to make a study of the spinal cord by examining a

*Address delivered (in French) before the Société psychanalytique de Paris, April, 1927.

cross-section at any given point, without instituting any inquiry into the source of the fibers seen in section, we know that the result would be extremely unsatisfactory. A longitudinal investigation is quite as essential as a transverse one. The point is obvious here, but very few realize that a similar consideration is just as valid for any scientific study. To make a cross-section of any science, by which I mean to study the state of that science at a given moment of time, without tracing the history and evolution of the ideas composing it, can only result in a thoroughly erroneous perspective. This is particularly true in a rapidly growing branch of science such as psychoanalysis, one which also has the two peculiar features of revolutionary novelty and of being essentially the creation of a single mind. Any attempt to comprehend psychoanalysis by reading Freud's latest books alone would be foredoomed to failure.

The doctrine of the super-ego illustrates this thesis very well. I can remember how at various stages during the development of psychoanalysis interest has been concentrated for perhaps a couple of years on one aspect after another, of course in connection with the latest discoveries of the moment. Thus, for example, towards the end of the first decade of this century everyone was absorbed in the problems of symbolism, especially from the practical interpretative point of view. New symbols were being discovered daily and light thrown on the points of association that were responsible for the forging of the symbolic links. One might well have thought in those days that the prime task of psychoanalysis was to learn how to interpret symbols, and that the rest would follow as a matter of course. After that came the turn of narcissism. This became the center of interest and all problems had to be put in terms of it. There followed the realization of how important were the problems of resistance, and in favor of this questions of mere interpretation were subordinated to the background. You will observe that I am not at all sketching here the order of development of important psychoanalytic ideas; I am merely mentioning two or three topics that have successively engaged, and rightly so, the forefront of attention. And it is necessary, further, to remark that these happenings were not at all matters of scientific fashion, of temporary interest. They all represented legitimate stages by which the center of interest passed from one piece of insight to the next in the gradual unfolding of our knowledge. Each successive stage was wider, deeper and richer than the preceding one; it did not replace the former, it simply embraced it in a more comprehensive fashion and set it

in a more correct perspective, but it could itself be only imperfectly understood if no reference was made to the earlier one.

Now in the last three or four years interest has centered with great concentration on what is known as the psychology of the ego, and prominent among these is the problem of the super-ego, with the nearly related ones of guilt and punishment. To read the literature of the present time one might well think that psychoanalysis was essentially a matter of the super-ego alone. Thus a young and distinguished psychoanalyst, Alexander of Berlin, whose point of view has been justly corrected by Reich of Vienna, recently wrote that "The kernel of the whole psychology of neuroses is contained in the sentence that guilt can be expunged by suffering", a statement that I should certainly designate as one-sided.

One might very well gain the further impression that Freud's important writings of the last few years on this subject had so revolutionized psychoanalysis as almost to create it anew; indeed, people even talk openly of the old and the new psychoanalysis as though the one had replaced the other. I want, on the contrary, to show you to-day that there is the directest possible continuity between Freud's earlier and later work, even on this subject of the super-ego, and that the whole of his doctrine constitutes an organic whole, though one ever in the course of evolution and expansion.

There are, as we shall note presently, a great many obscure problems in regard to the super-ego, its genesis and functions, but a certain number of considerations may be taken as being already well established. Thus, it arises through identification with one or both of the parents in connection with the child's conflict over the Oedipus complex—it has, indeed, been neatly designated by Freud as the heir of the Oedipus complex—and its most prominent function is to exert a constant criticism against any tendency of the ego to admit towards consciousness the derivatives of infantile sexuality, *i.e.*, the repressed unconscious. But is all this so completely novel? From the very beginning of his work Freud pictured the conflicts he had to deal with as arising between the repressed sexual impulses, mainly of infantile origin, on the one hand, and the collection of inhibitions of moral, social, aesthetic and ethical origin on the other. It was always plain that, even though some of these inhibitions may represent inherited tendencies—a matter extremely hard to prove—the most obvious source of them was the influence of the adult members of the child's environment, therefore typically one or both of the parents.

So that the two conceptions which we have selected as being the outstanding attributes of the super-ego are both present in embryonal form in Freud's earliest writings.

It will be remembered that at this time Freud, who was then chiefly occupied with following the ramifications of the various libidinal impulses, was content to establish a distinction between these and what he termed the ego instincts. The ego, or nonsexual instincts, which included the moral and aesthetic tendencies responsible for repression, were for some years taken for granted as a datum the investigation of which had to be reserved till later. The first step in this direction was taken when Freud was compelled to recognize that libidinal impulses previously directed outwards in the familiar way could in special circumstances, notably when subject to privation and repression, be turned inwards and become applied to the person's self as though it were a love-object. Reconstruction of the infant's mentality led to the further conclusion that this process reëstablished a primary situation, for the child's ego had been its first love-object, so that the turning inwards of alloerotic impulses really constituted a return of them to their original source. One differentiated in this way between primary and secondary narcissism, and, incidentally, one gradually learned that the narcissism met with clinically, which so often interposed a barrier against our therapeutic endeavors, was for the greater part of a secondary nature. An inevitable corollary of these conclusions was that the ego itself contained libidinal components, though Freud has sometimes termed them "desexualized" on the ground that no true sexual goal is to be discerned; so that the originally sharp distinction between the sexual and nonsexual parts of the psyche became somewhat blurred.

About the same time Freud formulated his conception of the "ego-ideal". The ego-ideal replaces the actual ego as the object of narcissistic libido when the imperfections of the actual ego become evident. Satisfaction of this libido then occurs only when the ideal is realized, or at all events approached, by the ego itself. When there is failure in this respect, when there is a considerable gap between the ego and its ideal, there results a state of dissatisfaction or even anxiety. A special institution criticizes the ego and measures its relation to the ego-ideal. Freud was at first inclined to postulate a separate existence for this institution, under the name of conscience, and to distinguish it from the ego-ideal itself, but later he fused both conceptions together in that of the super-ego. Conscience plays a predominant part in the criticizing function just mentioned, in the censorship exercised

over the latent content of dreams, and in the stimulus it gives to the ego in respect of repression of forbidden impulses. In his essay on narcissism, published before the war, Freud stated that conscience represented fundamentally an incorporation of the critical influence of the parents, later on that of society at large. "Social anxiety" or dread of public opinion would thus be a derivative of dread of conscience, this in its turn a derivative of the infantile dread of losing the parents' love, which is but another aspect of dread of castration.

As all these fundamental features of what is now called ego-psychology have constituted an integral part of Freud's teaching for many years, one naturally asks in what then lies the essential novelty of his more recent work on this subject. The correct answer would be, in the remarkable amplification and application of these fundamental principles, but I can put the matter more shortly and vividly by saying that the chief novelty resides in the doctrine that the most important part of the super-ego, and perhaps even of the ego also, is quite unconscious. This discovery gave rise at once to a fund of new problems. The very fact of the unconsciousness itself, for instance, makes one wonder what other reasons than the familiar one of repression can there be for any mental process being unconscious, or alternatively, what particular conditions of repression could be present to account for the most moral part of our personality, namely the super-ego, being unconscious. Perhaps light may be obtained in regard to this question by bearing in mind the distinction between the conscious and the unconscious parts of the super-ego. Examination of the differences between them shows that the conscious part is much more completely accepted by the conscious ego, very much corresponding, in fact, to what is commonly meant by the word "conscience", and that it bears traces of many influences that have helped to form it. Considerations of reality, with knowledge of actual life, have been allowed for in its formation. The unconscious part of the super-ego, on the other hand, presents a much more irrational appearance, *i.e.*, one unrelated to reality; it may easily, for instance, forbid a man to have intercourse with his own wife as though it were a heinous sin. Investigation shows two further characteristic features. The influences going to shape it have been derived from far fewer sources, and predominantly from the parents, and secondly, in correspondence with this fact, the attitudes and judgments composing it are those of the young child. These latter features explain the first one: it is because the unconscious part of the super-ego has remained on an infantile level. Con-

science is a part of the personality that needs to undergo a process of development as well as any other part, and yet there must be certain circumstances in which this development is prevented from taking place. We are familiar with this inhibition of development in other spheres, notably in the psychosexual, and are accustomed to ascribe it to isolation (or *désagrégation*) of the function concerned, one which Freud has taught us to recognize is the result of repression. These considerations would lead us to expect that the unconsciousness of the deep part of the super-ego may similarly be due to some process akin to repression, probably some defensive reaction against the pain of guilt.

The older unconscious part of the super-ego has important bearings on our therapeutic work, and indeed it was in this purely practical connection that its significance was first discovered. In certain cases Freud met with what he terms the "negative therapeutic reaction"; that is, that after a piece of analytic work that is usually followed by relief the patient's condition, his symptoms and sufferings, became unexpectedly worse. The explanation must be that something in the patient does not allow him to take advantage of this opportunity for subjective improvement, but at once imposes, as though it were a penance, an increased degree of suffering. The process bringing this about does not appear to the patient as any form of guilt, but purely as increased suffering. The cases presenting this untoward reaction offer the greatest difficulties to the analyst and invariably necessitate very long analyses, generally at least three years. Their occurrence provides one more reason why one should always be very cautious in giving a prognosis at the start of psychoanalytic treatment, especially on the question of duration, for one has many disagreeable surprises at discovering the reaction where one had had no grounds for anticipating it at the start. It would be of great diagnostic and prognostic value if we were in a position to recognize this type of case early, but at present this is not always possible. In my experience it is rarer in hysteria, though I have met it even here, than in the obsessional neurosis, but one should most of all be on the lookout for it in character analyses. As is well known, these have often to be undertaken in the so-called normal, that is, in people who have no neurotic symptoms and show mental organization, though formed on an unsound basis, that has been quite adequate for the contingencies of life. I may quote, for example, the case of a medical candidate who came to me to be trained for analytic work, and who expected to complete the self-analysis in a year or so. Beyond one slight fear there were no neurotic symp-

toms whatever and the only danger signal was that the individual was evidently of an inhibited type. Actually the analysis presented the greatest difficulties and took more than four years.

I am not going to say much about the relation of guilt and self-inflicted punishment to neurosis, because this is at the moment a hotly debated matter in psychoanalytical circles and the whole question is far from being cleared up. Though the very existence of the relationship has been doubted by some analysts, I would express my own conviction not only that it exists, but that it is one of great importance. We see this most clearly perhaps in the obsessional neurosis, where the pathological manifestations often occur in pairs, the first of which symbolizes some libidinal gratification and the second a reaction against this. It is true that these secondary reactions may be purely defensive in kind, having a sort of apotropaeic attitude towards the forbidden impulse, and some writers have confounded this with self-punishment proper. But there are instances enough where the reaction is evidently retaliatory and so must be styled a true self-punishment. Alexander maintains that such a self-punishment, symbolically inflicted of course, may precede a libidinal manifestation in a causative way, this naturally being also expressed as a rule in a disguised fashion, *e.g.*, an ordinary neurotic symptom. He thinks that the preliminary penance is performed so as to afford an excuse for the subsequent gratification, as though the censorship had been bribed in this way to relax somewhat its vigilance. If further investigation confirms this hypothesis it will evidently have important therapeutic reverberations.

I should like at least to point out that in approaching any of these problems it is desirable to become clear about some distinctions that are often obscured. Thus under the name of "self-punishment tendency" two opposite tendencies are sometimes confounded, namely that to punish and that to seek punishment. The infliction of pain of this sort, the familiar "stings of conscience", is a definite active process. The most obvious motive actuating it is the defensive one of influencing another part of the mind, typically the ego, in such a way as to inhibit forbidden impulses which it is imagined might provoke external danger (castration fear). But there is little doubt that the tendency may become secondarily sexualized, even if it is not always of this nature in part, and so achieve a sadistic aim in itself. The natural response to this tendency is pain, the sense of guilt, which the ego seeks in various ways to evade. It can do so by complicated mechanisms of flight and defense, by retaliatory acts of revolt

(*e.g.*, aggression against the ego-ideal), or by the dangerous avenue of acceptance, which implies both penance and inhibition. The acceptance may itself become sexualized on masochistic lines, and it is this that constitutes the most typical "need for punishment"; when this occurs in any high degree it constitutes one of the most formidable obstacles to therapeutic success. Generally speaking it is more favorable if the process of acceptance is conscious, in the form of pain or sense of shame or guilt. The extension of it into a masochistic perversion is always more pronounced in the unconscious part of the ego, though it may invade the conscious part as well.

In addition to this distinction between active and passive, the wish to inflict and the wish for infliction, we have to effect two further ones. Either of these processes may emanate from the super-ego or by the ego, there being thus four possibilities in the situation. Again, the active aggressive process may be a direct derivative of the hortatory voices of the early environment, *i.e.*, the parents, or it may, on the contrary, be directed against these. This gives us therefore eight possible permutations, and we are very far from exhausting the various factors concerned in the whole process. The simplest, and probably most typical case is when the active punishing tendency emanates from the super-ego at the dictate, ultimately, of the parental exhortation and is directed against the ego, but, as we have just seen, this represents only one of many possibilities. Nor should it be forgotten that several of these possibilities may be at work at different times in the same individual, so that the task of unravelling the true meaning of one of these manifestations is often extremely difficult.

It is time to turn to the question of genesis, to consider how the super-ego arises and of what it is composed. As was remarked earlier in this paper, we may take it as established that the super-ego arises through identification with a parent in connection with the Oedipus conflict, but at this point the questions begin. Even this initial sentence needs to be qualified by the consideration that there is probably what may be termed a presuper-ego stage of development on the analogy with the expressions pregenital and precastration stages. It is easy to divine, by a simple process of exclusion, that this, like the other two just mentioned, must be concerned with the alimentary and excretory functions. Leaving this matter, however, which has been very little investigated up to the present, we naturally inquire into what is really meant by the expression "identification with a parent", what is the nature of the process, what are the conditions or motives for its occur-

rence, and what factors are responsible for the important variations that we meet with clinically.

It is sometimes said that it is the super-ego which is responsible for the overcoming of the Oedipus complex, for the surrender of the incestuous wishes. Strictly speaking, it is not the cause of this repression so much as the *method* by means of which it is brought about. For the causes themselves we have to look in a still earlier stage of development, and there is little doubt that the dynamic factor is the emotion of fear. Freud, in his recent book "Hemmung, Symptom und Angst", has shown illuminatingly that it is possible to reach further back here than the definite castration complex with which we are so familiar. This is preceded by a more generalized state in which the infant's ego responds to privation (Versagung), or the lack of erotic gratification, by anxiety or anger. It is probable that the mere withholding of such gratification, even if involuntary, is always felt by the child to be an inimical act and one expressive of disapproval of its demands. We see here the dawn of what will later become guilt, but it is only this when the external privation is definitely an active process on the parent's part.

In this connection there is an important difference between the two sexes, one that has far-reaching consequences in later life. With the boy the idea of active interference rapidly becomes focused in the form of a castration fear; with the girl it remains much more on the plane of simple privation. With the boy the fear is normally displaced onto the father, who is felt to be more completely inimical to his sexual life than is the mother; with the girl the fear is largely associated with the father's withholding of gratification. The result is that the girl remains much more sensitive than the boy to possible disapproval on the part of the love-object, as every man of the world knows. These facts explain why it is that the man has played the predominating part historically in the development of moral and religious codes. It is well known to you, however, that the respective parts I have mentioned are modified in the case of homosexuality. Here the father plays an even more important part in the development of the girl's super-ego, but a less important one in that of the boy's; the boy's castration fear is here related to the mother, and the girl's general fear, related to the father, becomes of a definite castration nature.

We are beginning at this point to embark on a number of vexed questions, any one of which would need a special lecture for the proper elucidation of it, but before closing I should like to call your attention to one remaining matter. When one speaks of the

child's incorporating the loved object into his own ego by way of identification, and thus erecting a modified ego, the nucleus of the super-ego, this happens as the result of forced renunciation on account of the inaccessibility of the object. But it does not necessarily follow that the object thus incorporated is the primary love-object; indeed normally this is not so. For identification to take place there must, in my experience, be present an attitude of ambivalence towards the person concerned. Normally a boy's ambivalence towards his mother is not sharp enough for this to happen; his identification is therefore mainly with his father. Similarly the normal girl's identification is mainly with her mother, though the other parent plays with her a larger part than with the boy. In homosexuality the boy's identification is mainly with his mother, the girl's almost entirely with her father. We come thus to the general law that the super-ego is in all cases of predominantly homosexual origin, *i.e.*, it arises from identification with the parent of the same sex as the child feels itself to be (not necessarily its actual sex).

In this paper I have offered only a sketch of a complicated subject, and have answered very few of the questions I have put. But my object was to lay stress on some of the guiding principles that lie behind the investigation of the detailed problems, and above all to give you some sense of the continuity that resides in the development of the conception of the super-ego.

DIFFERENCES BETWEEN COLLEGE STUDENTS AND THEIR ELDERS IN STANDARDS OF CONDUCT

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FOR college youth the standard of abstract right and wrong is dead, according to Professor Avis Carlson.* Not only have the concepts of "sin", "wickedness", "piety" and "righteousness" completely lost meaning for the modern young person, but the authority upon which the right and wrong standard is based—the authority of church, home, school, and state—is fast waning. Since the right and wrong standard has lost its appeal Miss Carlson thinks it behooves society to set up a new standard for its rudderless youth which will offer an appeal for conduct. Public opinion is too fickle and divided to serve as an authority, she points out, and the social standard is too sentimental, romantic, and idealistic to make an appeal. The standard of prudence and practicality now holds sway, but its dullness and its wastefulness make it far from desirable.

The aesthetic standard, at once more positive and more adventurous, is the one Miss Carlson would substitute for the standards of righteousness. She produces observational evidence to the effect that young people have already adopted the aesthetic standard, in that they hate cheapness and sham in dress and cry down unfairness and poor sportsmanship, and she believes them ready to adopt a standard of aesthetic morality.

The aim of the present study was to discover on which of four standards college students most frequently claimed to base their conduct, and to determine whether the standard most frequently chosen differed for grandparents, parents and youth.

We devised a multiple-choice questionnaire of fifteen behavior situations, to each of which four choices of answer were given. One of these answers dealt with the situation according to the right and wrong standard, another according to the standard of prudence or intelligent judgment, a third according to the standard set by public opinion, or of doing what the crowd does, and a fourth according to the aesthetic standard. A copy of the questionnaire and scoring key is printed at the end of this paper. The

* Avis Carlson: "Wanted: A Substitute for Righteousness." *Harpers Magazine*, January, 1927, V. 154, pp. 148–157.

questionnaire is brief and inadequate in many respects, and some objections may be raised against the scoring key. Furthermore it may be charged that certain of the questions which are especially pertinent for students are irrelevant for their elders. Questions on creating, studying, and petting would probably not occur in a list of conduct questions made out for parents and grandparents. The merit of this questionnaire lies in the fact that it was made out by students for the use of students. But the results from its use on older age groups are quite consistent.

The questionnaire was given to five different groups: college students, parents, grandparents, university professors, and social workers. The college group comprised 30 men and 30 women, most of whom were of sophomore standing, and were co-students in a course in elementary psychology laboratory. The groups of parents and grandparents were obtained at social gatherings of our parents' and grandparents' friends; 21 fathers, 27 mothers, 13 grandfathers and 21 grandmothers were thus obtained. Only 13 university professors and 7 social workers took the test.

The average number of answers made by each group on the basis of each standard is tabulated below (Table I). See also Figs. 1 and 2.

TABLE I. AVERAGE NUMBER OF ANSWERS BASED ON EACH STANDARD BY DIFFERENT GROUPS

	Average Number of Answers on:			
	Standard of Right and Wrong	Standard of Aesthetics	Standard of Prudence	Standard of Public Opinion
Grandfathers	6.5	2.6	3.0	1.8
Grandmothers	8	2.3	3.6	1.8
Grandparents	7.2	2.4	3.3	1.8
Fathers	6	2.7	4.4	2
Mothers	6	3	4.1	1.5
Parents	6	2.8	4.2	1.8
College men	3	3.1	5.9	2.9
College women	3.3	4.6	4.6	2.3
College students	3.1	3.8	5.2	2.6
University professors	4.6	4.4	3.7	1.5
Social workers	4.6	3.6	6.8	1.1

From the graphs and table it is evident that both grandparents and parents claim to solve most of these 15 problems of conduct on the basis of the right and wrong standard; the other standards are preferred in the order prudence or intelligent judgment, aesthetic appeal, and public opinion, or social approval. College youth prefers to make its decisions according to the standard of prudence or intelligent judgment, its second choice is the aesthetic

standard, its third is the standard of right and wrong, and its fourth choice is that of public opinion. Among social workers the preferred standard is that of prudence. The judgments of university professors are almost equally divided among the right and wrong, the aesthetic, and the prudent standards. The number of cases in the last two groups is too small to warrant definite conclusions, however.

Slight sex differences appear within each age group, the most striking of which is that college women prefer the aesthetic standard more than do college men, and they prefer the standard of prudence less than do college men. We surmise that the explanation of this difference lies in the fact that aesthetic appeal is still considered by the men a bit effeminate, and not quite so manly as the standard of prudence or intelligent judgment.

Further evidence that college students differ from their elders in standards of conduct is presented in Table II. This table gives the difference between the number of questions answered on each basis by different age groups, the difference divided by its probable error, and the chances in 100 that the difference will lie in the same direction upon repetition of the experiment.

TABLE II. SIGNIFICANCE OF THE DIFFERENCE BETWEEN AGE GROUPS

Groups Compared	Standards: Right and Wrong Diff.	D P.E.$_D$	Ch's in 100	Aesthetics Diff.	D P.E.$_D$	Ch's in 100	Prudence Diff.	D P.E.$_D$	Ch's in 100	Public Opinion Diff.	D P.E.$_D$	Ch's in 100
Grandparents and youth.....	4.1	4.02	100	1.4	2.1	92	1.9	2.5	95	.8	1.1	77
Parents and youth	2.9	3.08	98	1	1.6	86	1	1.4	83	.8	1.1	77
Grandparents and parents.....	1.2	2.2	93	.4	1.2	79	.9	2.7	97	0	0	50

The greatest differences appear between grandparents and college students; between these two age groups the difference is most significant on the right and wrong standard, and next most significant on the standard of prudence. Parents stand midway between students and grandparents on all of the standards; they differ most significantly from the student group on the standard of right and wrong, and they are most significantly different from the group of grandparents on the standard of prudence. These results tend to confirm Professor Carlson's theory that the standard of right and wrong as a basis for conduct is rapidly dying and that the standard of prudence is being set up in its place. The standard of aesthetic morality is more preferred by college students than by parents or grandparents, and has already been

accepted by college women almost to the same extent as has the standard of prudence.

We realize that the people who answered these questionnaires probably do not base their actions on the standards on which they claim to base them. They are doubtless far more swayed by public opinion and far less activated by cool reason or prudence than their answers would indicate.

Although their answers are a product of rationalization, nevertheless it is likely that individuals can be appealed to by the standard on which they rationalize their conduct, whereas an appeal emphasizing the rightness or wrongness of an act might influence grandparents and parents, an appeal to college youth on the basis of prudence or aesthetics would probably be more effective.

This study does not reveal whether the standards of conduct have actually changed within the last four decades, or whether the standards of youth are always different from those of middle and old age. It may be that if the same students were to answer these questions twenty years hence, their answers would approximate those of the parents in this study.

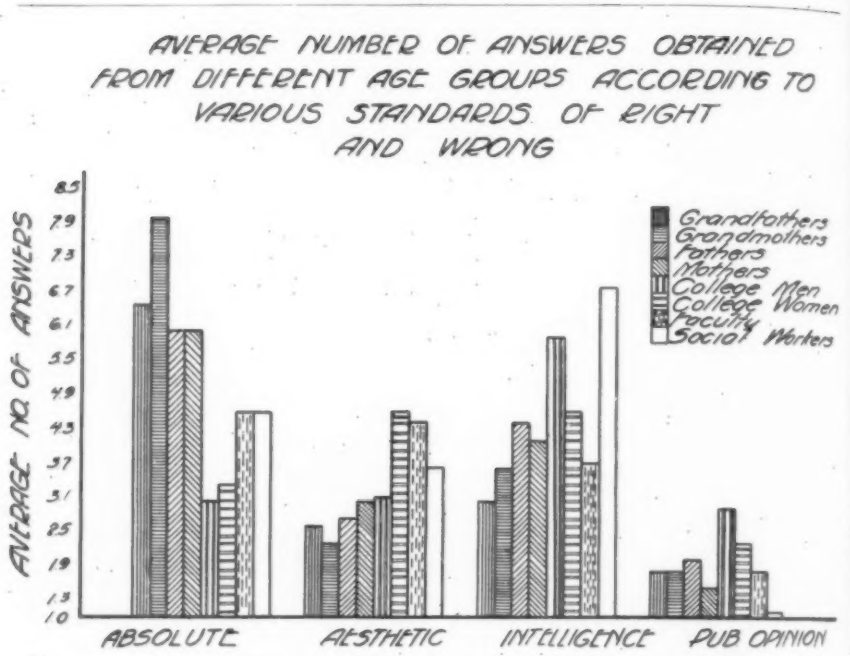
To conclude:

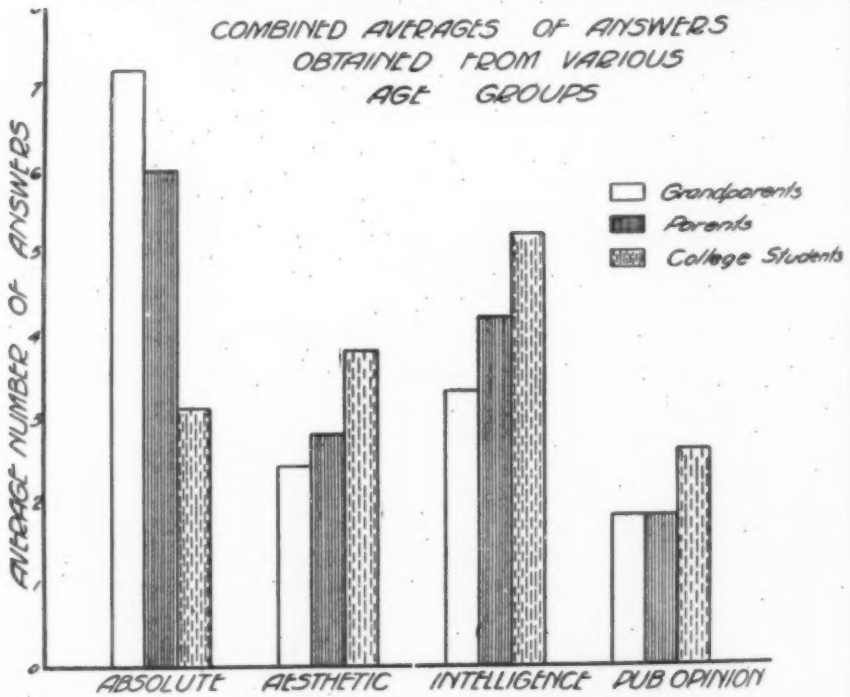
1. College students differ from parents and grandparents in the standards on which they base their conduct, in that they prefer the standards of prudence and aesthetics to that of right and wrong.

2. The greatest differences in standards of conduct occur between age groups rather than between sex groups.

3. All groups tend to eschew the standard of public opinion.

4. Since they make prudence and aesthetics their chosen standards for rationalizing their conduct college students probably could be appealed to on the basis of those standards far more effectively than on the standard of right and wrong.


AVERAGE NUMBER OF ANSWERS OBTAINED FROM DIFFERENT AGE GROUPS ACCORDING TO VARIOUS STANDARDS OF RIGHT AND WRONG
AVERAGE NO OF ANSWERS
85
79
73
67
61
55
49
43
37
31
25
19
13
10
Grandfathers
Grandmothers
Fathers
Mothers
College Men
College Women
Faculty
Social Workers
ABSOLUTE
AESTHETIC
INTELLIGENCE
PUB OPINION


COMBINED AVERAGES OF ANSWERS OBTAINED FROM VARIOUS AGE GROUPS
AVERAGE NUMBER OF ANSWERS
0
1
2
3
4
5
6
7
Grandparents
Parents
College Students
ABSOLUTE
AESTHETIC
INTELLIGENCE
PUB OPINION

SOCIAL REACTIONS ANALYSIS AND KEY*

DIRECTIONS: Answer these questions by underlining the one phrase in each group which most often characterizes your reactions. If you have difficulty in choosing between two alternatives you may underline both, but mark the alternative which usually has more weight in determining your conduct as *choice 1.* If none of the choices describe your behavior under a situation, describe it briefly in your own words. Remember you are to mark the one which most often determines your action, rather than the one which you think should determine your action.

1. In general do you choose your friends upon the basis of: (1) good sportsmanship; s (2) intelligence; i (3) morality; r (4) culture and refinement? a.

2. Which of these characteristics do you consider most disagreeable in a person: (1) immorality; r (2) stupidity; i (3) lack of appreciation of "the beautiful, the good, and the true"; a (4) lack of sociability? s.

3. If you were tempted to steal a ten dollar bill which of these reasons would more likely persuade you not to do it: (1) that you would be censured by public opinion if discovered; s (2) that you would be sure to suffer legal consequences if caught; i (3) that you would find stealing incongruous with your ideas of dignity and order of life; a (4) that it is absolutely wrong to steal? r.

4. Which of the following types of persons are you least tolerant of: (1) a cheap skate who does not get by socially; a (2) a thoroughgoing scoundrel; r (3) a person who is so "dumb" that he lacks all appreciation of the subtle and fine things of life; i (4) a person who is out for what will give him success, regardless of his fellow beings? s.

5. For which of the following reasons do you attend church: (1) that the sermon is of value to you; i (2) that the service is beautiful; a (3) that church going is socially the approved thing to do; s (4) that you believe it is right to go to church? r.

6. For which of these reasons are you attending the university (send your son or daughter to college): (1) because it is socially "the thing"; s (2) to gain an opportunity for becoming acquainted with the finer and more cultural aspects of life; a (3) because a college education is necessary for success in life; i (4) because you think that as an educated person you will be of more service to humanity? r.

7. If you were at a party where everyone else was drinking which of the following things would you do: (1) refuse absolutely because you believed it wrong; r (2) drink with the crowd; s (3) refuse because the actions resulting from intoxication would be distasteful to you; a (4) refuse because you believed it unwise or unhealthful to drink alcohol? i.

8. If you were in a class in which a majority were cheating on an

* Answer on the basis of the right and wrong standard is marked r.
Answer on the basis of the aesthetic standard is marked a.
Answer on the basis of the standard of prudence or intelligent judgment, i.
Answer on the basis of the standard of public opinion or socially approved, s.

examination would you: (1) cheat just a little; s (2) not cheat because cheating is wrong; r (3) not cheat, for fear of getting expelled if caught; i (4) not cheat because cheating is incompatible with your idea of dignified conduct? a.

9. If friends asked you to play golf or tennis on Sunday morning would you: (1) go with them because you would not like to be called a "bad sport"; s (2) go with them because you enjoy the beauty of the outdoors; a (3) refuse and go to church because you believed it the only right thing to do; r (4) go to church first and play golf later because it is well to be an all-round person spiritually as well as socially and physically? i.

10. Which of these policies do you follow in doing your school work: (1) place studying before pleasure; r (2) place pleasure before studying; a (3) study as much as is necessary to win the approval of your parents who are making sacrifices to send you through school; i (4) take as much time for pleasure as "getting by" in your studies allows you? s.

11. If you were driving a car and were late for an engagement would you: (1) speed when no policeman was near disregarding stop and go signs; i (2) keep within speed limits even though no "cop" was near; r (3) speed recklessly so long as you could "get by"; s (4) speed only because a late arrival would be embarrassing? a.

12. If you were in a "petting party" would you: (1) pet also for fear of being called a "poor sport"; s (2) refuse to pet because you consider it wrong; r (3) refuse to pet because it is "awfully poor taste"; a (4) pet because everyone else did, and you see no logical reason why you shouldn't? i.

13. If you were asked by a friend to admire a dress which you thought unbecoming and in poor taste would you: (1) frankly admit that you did not like it because it is right to tell the truth; r (2) say you did not like it because it offended your eye for the beautiful; a (3) say it was beautiful in order not to hurt her feelings; s (4) be noncommittal about it? i.

14. If friends of yours were contemplating a divorce on what ground would you try to dissuade them because: (1) divorce is contrary to your standards of right and wrong; r (2) divorce would interfere with the welfare and happiness of their children; i (3) the divorce would create an unbearable scandal; a (4) divorce would cause them to lose caste socially? s.

15. Upon which of the following criteria do you base your habitual conduct: (1) absolute right and wrong; r (2) sense of fineness, culture and good taste; a (3) intelligent and practical actions; i (4) accepted public opinion and practice? s.

THE MENTAL HEALTH PROBLEM IN THE COLLEGE

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DURING the last few years there has been a steadily growing interest in the problem of preserving the mental health of the students in our schools and colleges and a tendency to seek the aid of the psychiatrist in dealing with this problem. In accepting positions as mental health officers in our institutions of learning, psychiatrists are branching out into an entirely new field of work and one that promises to be of great importance. In this new and important development, the first thing to do is, obviously, to make a survey of the field with a view to forming some notion of the nature and extent of the work to be done and the best way of doing it.

Dartmouth was, as far as I know, the first educational institution in this or any other country to employ the full-time services of a psychiatrist and when I accepted the position of "Consultant in Mental Hygiene" in this college in the fall of nineteen hundred and twenty-four, I had very little notion either as to the kind of problems I was going to encounter or as to how I was to deal with them. I was not even sure that the college had need of a psychiatrist. I, therefore, felt that at the outset my work was to be looked upon as a piece of research. My first job was to size up the situation, to find out what work was to be done and the best way of going about it.

In tackling the problem of mental ill health in the college, the first question to consider is the cause of such ill health. Only after we have come to some conclusion on this question, is it possible to discuss intelligently the question of what we should do about it. I shall, therefore, begin by explaining, as briefly as possible, what from my case studies here at Dartmouth, I have come to regard as the chief causes of mental ill health, after which I shall pass on to consider the conclusions to which these observations would seem to lead as to the nature of the mental health problem in the college and the way in which we must deal with it.

Broadly speaking, there are four outstanding causes of mental ill health in the college:

First—Failure on the part of the student to deal in a satisfactory way with the demands of his sexual instinct.

Second—Failure to make a satisfactory adjustment in the sphere of his social life.

Third—Failure to adjust himself satisfactorily in matters pertaining to his work in the college.

Fourth—Physical ailments of one kind or another.

These four sources of mental disharmony, I shall consider briefly in the order in which they are here named.

Failure to deal satisfactorily with the demands of the sexual instinct is a source of much mental stress and strain, not only amongst college students but amongst young people in all walks of life and this for reasons that should be clear to everyone.

In order that man may be impelled to procreate and so carry on the race, Nature has given him a sexual instinct. In order that he may not be deterred from the performance of this vitally important function of reproduction, either out of regard for his own safety, ease and comfort or from any other cause, Nature has made this instinct a tremendously strong one, so strong as to break down all barriers that may be set up against it and to render man, in certain situations at least, practically powerless to resist it.

But, if Nature has said in unqualified terms that man must perform those acts on which the continuation of human life depend, Society has said quite as emphatically that he must not, except under certain conditions frequently impossible to fulfill. It is expected that a man should be in a position to support a wife before he satisfies the demands of his sexual instinct. The average college student is, of course, unable even to support himself. The result is that between the uncompromising THOU SHALT of Nature and the equally uncompromising THOU SHALT NOT of society, he is in a bad way. There is produced a state of emotional or nervous strain under which every one who is exposed to it suffers more or less although the results are much more devastating in some cases than they are in others. The amount of injury suffered depends chiefly upon two things; the make up of the individual himself and, the environment in which he is placed, the care and wisdom or neglect and folly of those whose business it is to instruct and guide him through this period of his emotional development.

As for the problem of social adjustment, Man we know is a gregarious animal. He cannot attain his full mental development except in the society of his fellows and he cannot be happy in the

society of others unless he is in harmony with them. He has a craving to "belong", to be a member in good standing of a group of some sort. This craving is a very powerful one. I am not sure that in most people it is not even stronger than that of sex. At any rate, I do not believe there are very many who if called upon to make a deliberate choice between satisfying their sexual demands and retaining their places as respectable members of society, but would choose to retain their social position and let the demands of the sexual instinct remain unsatisfied. Moreover, there can be no doubt that failure to satisfy our normal social demands is one of the most important causes of mental ill health. The man who lives entirely by himself or for himself is probably never a contented or happy one. Not only does he suffer the pain of loneliness, he is very likely to develop a suspicious and antagonistic attitude toward his fellows. He comes to think that people are against him and his feelings of antagonism and ideas of self-reference not infrequently grow in strength and complexity until we have, in course of time, well developed ideas of persecution. The man who lives away from his fellows is likely to grow queer in his thinking, queer in his behavior. A large proportion of the chronic cases in our hospitals for mental disease are cases of people with ingrown personalities, people who gradually have gotten out of contact and out of harmony with their fellow men, until finally they have become quite incapable of adjusting themselves to society or of getting along anywhere outside of an institution.

Now, in the ordinary large American college as it is organized today, many students experience a great deal of difficulty in satisfying their social demands. At the beginning of his freshman year, this is likely to be true of the socially gifted as well as of the man not so well endowed with the things which make for popularity. The boy who has been an outstanding athlete, student or social light in the preparatory school from which he comes, finds to his dismay on entering college that he is nothing but an insignificant little freshman milling around amongst some six or seven hundred other freshmen. There is no group to which he belongs and it seems that no one knows or cares anything about him. He feels himself a homeless waif, a mere bit of flotsam drifting about the campus and very homesick and miserable in consequence. If, however, he is a good mixer and of a type that conforms to campus standards, he soon makes friends and finds a place for himself, so we need not waste a great deal of concern upon him. But unfortunately for their own comfort and mental

health, not all the boys who come to college are of this type. There are some who are shy and sensitive, who do not make friends readily. They may have some defect or peculiarity which makes them unattractive to their fellows or the butt of ridicule. It may be a physical deformity of some sort or something about their appearance or manner that makes them seem effeminate. It may simply be that they are individuals who have had very few social advantages and are diffident and awkward. These individuals may go all the way through college and never fit in. At first they may put forth some effort to make friends but if their attempts are unsuccessful, if they feel that they are rebuffed, they will tend to shrink into themselves, they will become more self-conscious and perhaps develop the idea that they are disliked or despised. They will tend to grow bitter and antagonistic to their fellows and to the college in which they do not feel they have any real part.

The fact that there is so much social life in college, so many gay times, only makes the situation worse as far as these men are concerned, for it serves to call their attention to what they are missing. It makes them more clearly conscious of the fact that they do not belong. A particularly unfortunate factor in this situation is the fraternity system. It is felt that to be a social success in college one must make a good fraternity. A very considerable percentage of the men in college of necessity fail to make any fraternity at all and have therefore brought home to them clearly and unmistakably the fact that they are social failures. What makes this particularly bad is, of course, the fact that the men who are thus left out are the shy and awkward individuals, the poor mixers, the very men who are most urgently in need of the companionship and social facility a fraternity is supposed to give. It is frequently said that the most valuable part of a college education is not what one learns in the classroom but the social contacts, the friends one makes, the rubbing off of sharp corners by contact with one's fellows. Whether the importance of this has not been exaggerated in the case of the average student who tends to be always with the crowd to the neglect of his studies, there can be no question that for the shy, seclusive individual, the man with more than the usual number of corners to rub off, it is of vital importance.

Consider next the third factor, the failure on the part of the student to find satisfaction in his work. In hospitals for mental disease, it is coming to be recognized today that perhaps the most valuable curative agent at our disposal is work. What is true in

our hospitals for the insane is also true in our clinics and in office practice. We are coming to see that mental health depends more on interesting and congenial occupation than, in all probability, on any other factor. "Occupational therapy", as it is called, has come to be recognized as a very important form of treatment by psychiatrists and neurologists today. The more intelligent men in private practice are coming to realize more and more clearly, that when a patient comes complaining of some nervous or mental symptom, one of the first things to do for him is to find him a job of some sort, that will take his mind off his troubles and make him feel he is of some use in the world and that life is really worth while. The tendency today is to lay everything to sex but all of us have sexual difficulties or have had sexual difficulties at some time or other in our lives. Why have we not all broken under them? Sexual dissatisfaction, although it may be present, is never the only factor in a case. One is very unlikely to develop any very serious nervous or mental disorder as a result of any sexual difficulty as long as he is fortunate enough to have work into which he can throw himself and which he feels to be really worth while.

One of the causes of mental disharmony in the college student, and from my experience at Dartmouth, I should say that it is by far the most important cause, is that so many of them do not find satisfaction in their work. If they were really absorbed in their work, if they were inspired to give themselves wholeheartedly to it, sex and lack of social success would not be productive of anywhere near as much mental disharmony as they now are.

The problem of why students so commonly fail to find satisfaction in their work, of why they take so little real interest in it, is a very complex one. There is one factor in the situation, however, which might be mentioned and which I feel is of fundamental importance.

In determining our attitude toward any game or form of work, probably nothing is so important as the degree of success we meet in it. We all of us like to do the things we can do well, the things at which we excel. We hate failure and we tend to develop a dislike for the things in which we fail. We find no pleasure in the things which we do poorly and in which we show to disadvantage in comparison with our fellows. While, however, we enjoy success, we get but little satisfaction in the success that is too easily won. The maximum amount of pleasure is found in the successful doing of that thing which calls out all our powers. The skilled marksman will find no satisfaction in shooting at the side

of a barn or the highly trained mathematician in doing a sum in simple arithmetic. To develop or maintain interest in any form of work, it is necessary that it should be hard enough to call forth our best efforts but not so difficult as to cause discouragement or the pain of failure. This is the first principle to bear in mind in attempting to develop interest in any kind of work. There is no form of activity, however trivial or worthless, in which men will not become interested if only they can once taste in it the satisfaction which comes from success. There is no kind of work, however important it may be, from which men will not turn if in it they experience only and always the pain of failure.

Now, as it happens, our whole system of college education, the chief purpose of which is generally supposed to be to develop an interest in things intellectual, is built with an almost total disregard of the operation of this principle. Bright students and dull, the quick superficial students and the slow but thorough ones, all are expected to spend four years in college and to cover approximately the same amount of ground in that time. In any course, the pace set, if the teacher correctly estimates the ability of his class, is what the so-called "average student" may be expected to do without unduly exerting himself. However, a pace such as this means that the brighter members of the class are not called upon to exert themselves. They are constantly being held back and forced to listen to explanations of points which are already clear to them and which are therefore dull and tiresome. As for the men at the other end of the scale, the dull students or slow thinkers, the pace being set for men of medium ability, they find it altogether too fast. To keep up with his class the dull student must put forth great effort and even then points are passed over before he is able to fully grasp them. So he becomes confused and discouraged. He loses interest in the subject and sinks into an attitude of bored indifference, resigning himself to the necessity of getting merely a vague general impression of what the thing is about, with a few outstanding facts and formulations painfully memorized in order to make it possible to wriggle through the examinations somehow and remain in college, until ultimately he obtains his much coveted degree and can kick up his heels and turn his back forever on all the hard, dry stuffing with which for four long years he has been obliged to cram himself in order to obtain it. So bright students and dull alike come to regard their studies with an attitude of bored indifference which tends to become the spirit of the college and to communicate itself even

to those men who but for the contaminating influence of their fellows would find a real interest in their work.

As for the conditions of physical ill health which affect the mental well being of the student, the most important of these is probably fatigue, which may occur as a result either of too much work or of too much play.

The ill effects of fatigue from overwork are most frequently seen in the freshman year, which most graduating students look back upon as their hardest year in college. Take the case, by no means infrequent, of the hard working but by no means brilliant lad who has been accustomed to hold a place somewhere near the head of the class in preparatory or high school by dint of consistent hard plugging. He comes to college where the level of scholastic ability is much higher than it was in the high school from which he came, the student body here being composed of the picked men from a great number of different high schools. The methods of instruction also are different from those to which he has been accustomed, the subject matter is new and unfamiliar and the assignments of work are much longer. As a result of all this, he finds the work at the outset extremely hard. He has always been accustomed in high school to come into class each day with his work well prepared. He is no longer able to do this. What happens? Being an ambitious and conscientious youngster, he cuts down on his recreation and hours of sleep and exercise. He works far into the night, striving to do that which he thinks is expected of him. But as he thus continues to draw upon his reserves of nervous energy, he grows fatigued. He becomes less efficient and, as the days go by, he finds that it takes a constantly increasing amount of time to get through his daily tasks. So he begins to sit up still later into the night in his efforts to keep abreast of the work which is steadily piling up against him.

This, of course, only adds to his fatigue and the work becomes in consequence just that much more difficult. A vicious circle has been set up. Then the boy begins to worry excessively over the fact that he is not doing satisfactory work. He fears that he is going to flunk out and be sent home in disgrace. He begins to wonder why it is that he has to struggle so hard to do what his fellow students seem to be doing with ease. He realizes that his mind is dull and sluggish and this does not make him feel any better. He becomes depressed, develops what he calls an "inferiority complex", thinks that he is good for nothing and that everybody looks down upon him. If this depression is severe enough,

he may become practically incapable of carrying on work of any kind and unless some one comes to his assistance, the chances are that he really will flunk out and return home to his family with his tail between his legs and his morale perhaps permanently damaged as a result of the experience through which he has passed.

So much for the causes of mental ill health in the college. There are, of course, many other factors which I might mention but these are quite enough to enable us to understand the nature of the problem with which we have to deal. The next question to consider is,—What can be done about it?

In so far as the obligation of the college to the student is concerned, the problem of the preservation of mental health involves three things:

First, Giving the student a healthy mental environment in which to live.

Second, Educating him in regard to the principles of Mental Hygiene so that he will be able to proceed intelligently in issues involving his own mental health or that of his fellows.

Third, Giving him such individual assistance as he may require in making his adjustments during the time that he is in college.

The first of these things is altogether too involved to be discussed in the space of a paper such as this. The second is one which has been discussed elsewhere in previous articles.[1] It is the third question with which we are here concerned.

In any educational institution, no matter how well conducted it may be, situations will be constantly arising in which students, if left to themselves, will tend to fall into unwholesome mental habits or will find themselves in difficulties of one kind or another in which they will have need of individual assistance and advice. Not only the student's immediate peace of mind, but also his ultimate well being, will depend on the correction of these bad habits, the solution of these problems. It is highly important, therefore, that the student should receive the help he needs and it would appear to be one of the duties of the psychiatrist to see that he gets it. How is he to do this?

The obvious procedure would seem to be for the psychiatrist to set certain hours when students may come and consult with him in regard to their troubles. This is what I have done at

[1] The Development of a Mental Hygiene Program in a College or University, JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY, Vol. xxi, No. 3, October-December, 1926, and The Problem of Mental Hygiene Courses for the College Student, *Mental Hygiene*.

Dartmouth. It is what probably every psychiatrist has done who has gone into this field of work. Now what can be said of the value of this conference work? How far does it go toward meeting existing needs and wherein does it fail?

Of the men who come to see me, there are some for whom I can do comparatively little. They suffer from defects in themselves or in their environment which I am powerless to change. Their troubles are due to causes which lie almost entirely beyond my control. There are others, however, who can be helped a great deal. There have been quite a few who, when they first came to me, were in such a condition that it seemed as if they would have to leave college. There were some who talked of suicide. Most of these students, sooner or later, succeeded in making a fairly good adjustment and up to the present time none of the men who sought my assistance have committed suicide. I do not know how much credit I am entitled to claim for the satisfactory outcome of these cases. Many of them, in all probability, would have pulled through sooner or later without assistance, but there is no doubt that in many cases the assistance received was of considerable importance, in some of them enough to make the diffierence between success and failure.

Conference work of this sort then is well worth doing, but does it adequately meet the needs of the student body or is it the best that we can give? That is a very different question. Let us see where this procedure fails to meet our needs and what can be done about it.

One serious defect in the present method is that under it the men who most need assistance commonly do not find their way to the psychiatrist at all, or else only after the trouble has reached a point where it is impossible to do much for them. For example, I was called out in the night recently to see a boy who was in a state of marked excitement which had developed evidently as a result of fatigue and emotional strain. This lad had shown signs of being in an abnormal mental condition for some time but there had been no one in contact with him who had recognized the need of doing anything about it. The result was that he did not come to my attention until he had reached a stage where there was nothing to be done except telegraph for his father to come and take him away to some place where he could be kept under medical care. Another case was that of a young man who came to me in a state of depression the result of infected tonsils, fatigue and worry over his work. He was a lad of exceptional ability but

with no money who was working his way through college and carrying a much heavier load than was good for him. He struggled along by himself as long as possible and then came to me with his troubles. I sent him to the hospital where he got rid of his infected tonsils and had a chance to rest for a while. But he had gotten into such a depressed and anxious state of mind that he was quite unable to pull himself together. It was, therefore, necessary, for him to drop out of college for a full year. That year has now elapsed and he has not returned. It is doubtful if he ever will return to college and take his degree.

Another serious objection to the present method of doing conference work, is the difficulty of getting adequate information in regard to the cases with which one has to deal. To do satisfactory work, one requires always to know something of the personality of the patient he has to treat, the conditions under which he lives and the way in which he has been reacting to these conditions. One cannot always depend upon the patient to give this information himself because he sees things only from his own point of view. To get a thorough understanding of a case, it is necessary to supplement the patient's story with information obtained from other sources. As a rule, however, in the case of the student who comes to the college psychiatrist seeking help, outside information is hard to obtain. The boy has friends probably among his fellow students but he would not care to have the psychiatrist call them in and question them and as a rule it is impossible to find any member of the faculty who knows much about him. This of course is a very serious difficulty from the standpoint of treatment. Rational treatment of any trouble involves removal of the cause. The causes of a large percentage of the mental troubles one has to deal with in the college are to be found in the conditions under which the student lives. But how can one hope to correct these conditions when he cannot even find out what they are?

But even if the two difficulties just considered were disposed of, the psychiatrist would, under existing conditions, be unable to fully cope with the situation by reason of the fact that in a college such as Dartmouth there is much more work than any one, or any five psychiatrists for that matter, could possibly handle by their own unaided efforts. I am not infrequently asked how many problem cases there are in a college of the size of Dartmouth, to which I invariably reply that in a group of two thousand undergraduate students such as we have here, there are two thousand psychological problems. No one adjusts himself perfectly. Every

student is a problem case and needs a certain ~~about~~ amount of supervision and individual assistance, only, of course, some students are more poorly adjusted and need more help than others, and while there are some cases that can be disposed of in half an hour, there are others that require a great deal of time and attention.

By far the greater part of this personal work which the student requires could and should, of course, be done by some one other than the college psychiatrist, a junior member of the faculty perhaps, or in many cases, a fellow student. But, to make use of the services of such men, it is necessary first to build some organization through which they can be rendered available at such times and in such ways as they may be required.

With this analysis of the situation, let us proceed to consider the question of what is to be done about it.

As a means of getting in touch with the students who are in need of help, it might seem at first sight that the obvious course would be to enlist the coöperation of the teachers in the college, that the teachers could pick out and send to the psychiatrist the men in their courses who are in need of help. Such a plan is doubtless of some value and a certain number of students can, unquestionably be reached in this way. Unfortunately, however, teachers in our colleges today do not, for the most part, see much of their students outside of the classroom and although in the classroom one learns something about a man's intellectual attainments and interest in the subject taught, that, as a rule, is about all one does learn. It is hard to tell from seeing a man in class whether he is social or solitary in his habits, whether he sleeps well or ill, whether he is contented or miserable. The fact that a student appears dejected or indifferent at such a time does not in itself indicate that he is sinking into a depression. It may be merely due to the fact that he is not finding the class an interesting one. No, teacher, therefore, could possibly pick out, merely from seeing them in class, the men who are in need of help.

Another plan that has been suggested, is to have some method of systematically examining all the students in the college, of interviewing them one after another, and, by means of such examination, to sort out and send to the psychiatrist all who need his services. The same difficulty applies here, however, as in the method just considered. We have no tests we can use as a means of determining which men are in need of help and which are not. I do not know of any way in which even a well trained psychiatrist could go through a large body of students, separating the sheep

from the goats and there are no men with psychiatric training available for such work.

So neither of these methods can be looked upon as of much value. What then are we to do?

The first step toward finding an answer to this question might perhaps be to ask ourselves why in our colleges as they are today we know so little about the individual student with his individual problems. The cause of the trouble is to be found in the increase in size of the college and the lines along which it has developed. In the small college of earlier days, the instructors were personally acquainted with all their students and the students were intimately acquainted with each other. Everybody knew everybody else. The institution was a social unit. The problem of keeping track of the individual student which is, from our point of view, such an important one did not then exist. As the college grew in size, however, the homogeneity of the group began to be lost. It tended to break up into parts and it was obvious what the lines of cleavage would be. The men who entered at the same time, attended the same lectures and graduated together tended to draw into a group. The student body divided into classes. But today, in a college like Dartmouth with its two thousand students, the unity of the class has become little more than a tradition. A man may spend four years at college and graduate without coming to know, even by sight, many of the men who are in the same class with him. As a social unit in the college the "class" has ceased to function.

As the group has grown in size, as the students have lost contact with each other, they have also lost contact with their teachers. This loss of contact between student and teacher is rendered more pronounced by the fact that with the growth of the college and increase in the number of men on the faculty, the teachers have tended to specialize more and more. Where in the old days a man taught several subjects to a small number of students, he now teaches only one subject, or one phase of a subject, to a great many. Specialization has been described as a process as a result of which a man comes to know more and more about less and less until finally he knows everything about nothing. Whatever truth there may be in this definition, certainly the process of specialization has led to our teachers knowing less and less about their students until in many cases it amounts to almost nothing.

In earlier days a man spent many hours making a single pair of shoes. This pair of shoes had an individual interest for him. By the time they were finished, he knew them thoroughly. Each pair

had for him a character of its own. Today shoes are made in huge factories. Each shoe passes rapidly through a great many hands and no workman would recognize a pair of the shoes he had helped to make if he met them on his own feet. We are today using in our colleges the same methods of mass production we use in the making of shoes. The result is increased efficiency along certain lines but serious loss in others. The teacher, as a rule, knows more about the subject matter which he covers in his course but very much less about the man to whom he teaches it.

What can be done about this? We cannot scrap our large institutions of higher learning and go back to the smaller colleges of an earlier day. The only course open to us seems to be to break the college up into a number of units each having its own social life and to build an organization by means of which it will be possible to maintain the necessary personal contact with these units.

In developing such a plan, we of course must begin by deciding what this unit shall be. Where the men live in the college, the logical course to pursue would seem to be to make the dormitory the unit. Having decided to make the dormitory the unit, the next problem would be that of centering the social lives of the students in the dormitories to which they belong.

In order to draw the men together who occupy the same dormitory—to make of it a real unit—the best plan would be, wherever possible, for each dormitory to have its own dining room and recreation rooms and for all the students who occupy it to take their meals together there. It is wonderful what eating together at the same table three times a day and smoking together after dinner in the same lounge will do in the way of developing social intimacy between the members of a group. It would be desirable for the men occupying a dormitory to form a definite organization, to have a president, a treasurer and secretary and whatever other officers might be necessary, just as in any fraternity or club. In fact it would be a good thing to drop the term dormitory and call the unit by some other name, a name which would indicate that it was something more than a place in which to sleep. One might use the term, house, or even call it a club.

Interdormitory contests of various kinds would be another feeling of loyalty, each for his own particular group. Finally, means of drawing the men together and of inspiring them with a there should be as little as possible of shifting of men from one unit to another. It should be felt that when a man enters a given

unit at the beginning of his freshman year, he has entered an organization of which he is to remain a member as long as he retains any connection with the college of which this organization is a part. This is necessary by reason of the fact that one is not likely to be very deeply interested in the fellow members of a group which he feels is only a temporary one. When a man realizes that the men with whom he finds himself are men with whom he is to be associated during his entire college course, his attitude toward them will be a very different one indeed from what it would be if the association were for only a short period of time. There is established from the outset a feeling of interest and coöperation which is of benefit to all concerned. The upperclassman gets a feeling of responsibility for the freshman in his group which serves to steady hi mand make him more of a man, while the freshman gets from the start that sense of comradeship, of belonging to a group, which is so essential to ones happiness and peace of mind, and which is of such profound importance from the standpoint of mental health.

But, having broken our student body up into social units, the next problem is to maintain contact with them. How is this to be done?

As far as I can see the only way to do this would be by means of proctors or counselors and my suggestion would be to have the counselors work under the direction of the mental health officer. These counselors would be drawn from the younger members of the teaching staff of the college. The amount of teaching they would be called upon to do would be reduced to set them free for this work. There would be one counselor for say every hundred students. The counselor would live in the dormitory with his men, eat his meals in the dormitory dining room and smoke his after dinner pipe in the dormitory lounge. Thus it would be easy for him to become well acquainted and keep in constant touch with every member of the group committed to his care.

The mental health department of the college would then consist of the mental health officer of the college who should be a well trained psychiatrist responsible for the mental health of the student body as a whole and under him a group of counselors, each of whom would have charge of a unit of about a hundred men and would be responsible to the psychiatrist for the mental health of the students in his particular unit. Let us see how such an organization might be expected to function and the nature of the work it would do.

From a unit of one hundred students there would be, let us say, some thirty-odd men graduating or dropping out of college every year and some thirty-odd freshmen coming to take their places. It would be the duty of the counselor, at the beginning of each year, to become acquainted with each of the new men entering his group and to form as accurate an estimate as possible of what might be expected of him. As a means of helping him to do this, he should, of course, be provided with all data available. He should, for example, be informed as to what schools his men came from and what these schools had to say about them, also the results of medical examinations, college entrance examinations and any psychological tests which they might have been required to take.

After this initial stock-taking of the incoming members of his group, it would be the duty of the counselor to follow his men right through their college careers in order to see that their possibilities were realized. It would be comparatively easy for him to keep track of his men by reason of the fact that each student would remain in the same unit under the care of the same counselor during his entire stay in college. The system of placing the student under one officer during his freshman year and under another during his sophomore year, or of shifting men from group to group has this disadvantage that an officer no sooner begins to know the men under his care than they are shifted to some one else and he has to start in all over again. There is a great waste of time and effort. The officer is forever learning but never coming to a knowledge of the men committed to his care.

Whenever a counselor found that one of his men was not getting along as well as he should, that he was unhappy or inefficient in his work or in some way failing to adjust himself to his environment, it would be his duty to investigate the matter. If the trouble were a simple one, he could probably get to the bottom of it and set it right himself. If, however, he found it too difficult to handle alone, he would take it to the psychiatrist and the counselor and psychiatrist would work the problem out between them. Wherever it seemed necessary or desirable to do so, the psychiatrist would, of course, take the student under his own personal care.

The information on which the counselor would base his opinion as to whether a man was getting along satisfactorily, whether he needed help or not, he would obtain from several different sources. The first would be his own observation. He would be seeing the man daily at meals and in the living rooms of the house in which they both lived. If the man were depressed, uneasy, irritable,

seclusive in his habits or showed any other peculiarity, he would be very likely to notice it. If he did not observe it himself, someone else would and, in all probability, bring it to his attention. In the second place, if the counselor were the right kind of a man, if he were efficiently performing his duties as counselor, he would have the confidence of his students and when they were in trouble and needing help they would be very likely to come to him for it. Finally, there would be certain information of great value which could be supplied to him through official channels. For example, it would be necessary for the counselor to know how his men were getting along with their studies. If a man were falling low in his marks in any subject, or absenting himself from any of his classes, the counselor would want to know about it, so that he could find out the cause of the trouble and set it right. Also if a man got into any other form of difficulty likely to call for disciplinary action the counselor should be informed of it at the earliest possible moment. With the work in the college properly organized, all items of information such as these I have mentioned, and various others beside, would be sent promptly to the counselor without his asking for them as a matter of simple routine.

Under the system here outlined, by reason of his close contact with his men and intimate knowledge of them, the counselor would be in a favorable position to discover the fact if any trouble existed and to get at the cause of it. For the same reason, he would be in a favorable position to treat it, for he could advise the student from day to day and observe the effect of his advice. Also he would be in a position to enlist the services of the man's companions. Most students are kindly and helpful and would be willing to do a great deal in the way of helping their comrades if there were only some competent person at hand to point out to them what they should do. A large part of the mental hygiene work in the college could and should consist of getting the students to help themselves and to help one another.

Under this system, we should, as a matter of fact, have three classes of workers. First, the students themselves; second, the counselors, and third, the college psychiatrist or mental health officer, to whom would be referred the problems which the counselor and the students under him were unable to handle by themselves. Working under this system, the psychiatrist would have brought to him only such cases as were sufficiently difficult to require his personal attention. He would not be obliged to waste

his time on those simple problems and routine procedures which someone else after a little training could handle just as well.

Although it is unnecessary to go into full details as to the working out of a scheme such as this, there are two highly desirable procedures which it might be well to mention.

First: Records of the students should be kept by the counselors. These records should be on file in the psychiatrists office where sufficient clerical assistance should be provided to keep them in good order. Such records would be valuable, not merely for the information contained in them but also because they would make for better work on the part of the counselors. Scientific workers in all fields have found that good note taking is the first essential to accurate observation. If we wish to ensure that our counselors will study their men, it is highly important to have them keep notes on them. It would, of course, be part of the duty of the psychiatrist to supervise this work and see that the note taking was kept up to the highest possible standard.

Second: Staff conferences should be held at regular and frequent intervals—say once a week. In these staff conferences there would be discussed the various difficult cases and mental health problems which the counselors encountered in their work. Such conferences, if properly conducted, would not only be very instructive, the would also serve to stimulate and keep alive the interest of the counselors in their work.

Such is the tentative plan of organization I would suggest as a means of attacking the mental health problem which confronts us in our colleges to-day. Let us consider briefly its merits and demerits.

As to its merits, there are four which may be mentioned.

First: It affords a means of maintaining contact with the individual student which is the initial step toward the development of an adequate mental health program in the college, for without such contact and the knowledge it gives, we can neither know who needs help, why he needs help nor what should be done for him. As a matter of fact, the personal contact which a plan such as this gives and which, as far as I am able to see can be maintained in no other way, is essential not only to the preservation of the mental health of the student body—using the term mental health in the somewhat artificial and restricted sense in which it is commonly used—but is also essential to sound education. The purpose of a college education is to benefit the student, to send him out better equipped to cope with life than he was when he came in, or rather

than he would have been if he had not come to college. Under our present system, a great deal of time and effort is spent by the faculty of the college in building an elaborate organization for imparting knowledge and in the preparation of the material to be taught. Very little time is spent, however, in studying the student to see whether what he receives is doing him good or harm. The case is very much like that of the marksman who gives a great deal of time and thought to the mechanism of his gun and the ammunition he uses but never looks at his target to see where his shots are landing. To do good shooting you must keep your eye on the target. To do good educating you must keep your eye on the student and to keep your eye on the student you need some kind of machinery by means of which the course of each individual student may be followed through the entire length of his college career—and out into the world too for that matter, in order to see whether the things he learned while in college and the habits he developed were really what he required in order to fit him for life in that world.

Second: It gives the college psychiatrist the assistance he must have if he is to properly perform his task of taking care of the mental health of the student body. As I have tried to show, the mental health of the college cannot be properly cared for except by giving a great deal of attention to the individual student. This means much work that should not and cannot be done by the psychiatrist himself. The plan here outlined enlists the services of both students and instructors. It gives us an organized corps of assistants by whom this work can be efficiently performed.

Third: The plan of organization here suggested, involving as it does the breaking up of the student body into a number of social units, affords in itself a means of removing one of the most important causes of mental ill-health with which we have to deal, for, as I have already pointed out, one of the chief causes of mental ill-health in the college is failure on the part of the student to satisfy his social demands. I have pointed out how under existing conditions the shy and awkward student, the man with more than the usual number of rough corners to rub off, is likely to be left out in the cold and the serious ill effects to which this is likely to give rise. The scheme of organization here outlined would take care of this difficulty. The student on entering college would find himself one of a group of men who would be personally interested in him and with whom he would feel that he belonged. If, however, for any reason, he failed to shake down at once in this group, there would

be a counsellor close at hand to diagnose the trouble and smooth the way to a satisfactory adjustment.

Fourth: It would serve as a means of increasing the instructor's understanding of the college student and his needs. The mental health of the student, as we have seen, depends in the main on his environment, on the various influences which act upon him from day to day. But in the long run it is the instructor and not the college psychiatrist who has the biggest voice in saying what these influences shall be. It is the instructor chiefly who shapes the college environment in which the student must live. It is the instructor therefore who holds in his hands the forces on which the mental health of the student body depends. This being so, it is essential that he should be interested in the mental health problems of the college and should have as full and accurate a knowledge of them as possible. Under the scheme which has here been outlined, one instructor for every one hundred students, more or less, will serve as a counsellor in the mental health department, and in doing this he will be studying the student and his problems under the direction of a technically trained man who is devoting himself exclusively to this work. After he has served a certain length of time in this department, after he has had a certain amount of training and experience, the instructor will pass along and some younger man step into his shoes. So in course of time it will come about that a fairly large proportion of the faculty of the college will have had practical and systematic training in dealing with the mental health problems of the college and will have a much better understanding of the student and his needs because of it. It is obvious that what this scheme really does is to give a laboratory course in mental hygiene for the young men who are just entering the teaching profession.

So much for the advantages of the plan. Let us see what can be said against it. The chief objections likely to be raised are that it would be very expensive and involve such radical changes in the organization of the college as to place it beyond the range of possibility. What do these objections really amount to?

As for the expense, students must be fed and also have need of recreation rooms and these necessities can be provided just about as cheaply in the buildings in which they sleep as anywhere else. The plan here outlined would only be expensive where the existing college equipment had to be extensively remodelled in order to conform to it. In such cases, however, the plan need

not be adopted in its entirety or all at once. It could be introduced gradually and with such modifications and compromises as existing conditions rendered necessary.

But even if it should involve considerable expense and somewhat radical changes in the existing college organization, what of it? What else can one expect? No doubt it would be very nice if we could solve the problems of mental health in the college without either trouble or expense and without altering our point of view or manner of life. Unfortunately health of the mind, like health of the body and most other things worth having, is not to be obtained in this way. It would be very nice indeed if the doctor could give us some kind of medicine that would enable us to go on eating unwholesome food, living in unsanitary houses, keeping bad hours and abusing ourselves in a variety of ways and yet remain healthy ourselves and have our children grow up strong and healthy as well. But alas, no such medicine has ever been discovered. Similarly in the field of mental hygiene, if we are to have our young people healthy in mind, we must give them conditions conducive to mental health, we must give them the care and supervision they require.

Mental health is not to be preserved by the practice of psychoanalysis or any similar form of treatment. No intelligent person nowadays would think it necessary or desirable to dose himself with tonics and cathartics to keep his body in good condition. We have outgrown that sort of thing. It should be equally apparent that health of the mind is not to be preserved by "mental catharsis". Mental health does not depend upon muck raking in the "unconscious" for pervert sexual desires or memories of our intrauterine experiences as our psychoanalytic friends would have us believe. There are no psychological nostrums the mental healer can ladle out in his office that will take the place of right living. The aim of the mental health officer in the college should not be to build a huge clinic to which a constant stream of morbidly introspective students will keep running in order to talk about themselves and receive mental treatment of one kind or another but to bring about a state of affairs in the college in which there will be as few students as possible with mental ills to treat.

Improvement in the mental health of the college is only to be attained by giving the student a more healthy environment in which to live and teaching him how to conduct himself in it. In our various cities and towns we now have health officers whose business it is to protect the physical health of the inhabitants,

but no one would be so foolish as to expect the health officer to go off into a room by himself and there do something that would banish disease from the community without subjecting it to any expense or without modifying the peoples' manner of life. Physical health in our towns and cities has been improved through new and better waterworks and sewage disposal plants, through improvements in housing conditions and compulsory vaccination and a great variety of other far-reaching reforms that have cost a great deal of money, that have profoundly modified our way of living and that have, in many cases, aroused the bitterest opposition. It is absurd to suppose that the mental health problems in our schools and colleges or in society at large are ever to be solved in any other way than that in which we are solving the problems of physical health. Health of the mind, like health of the body, is only to be had as the result of great expenditure of money and effort and as the price of many changes in our established habits and beliefs. If we want mental health and the happiness it brings for ourselves and for our children, if we think that it is really worth having, we must be prepared to work for it and to pay the price that it will cost.

NOTE—In a recent paper (The Development of a Mental Hygiene Program in a College or University, JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY, Vol. XXI, No. 3, October–December, 1926), I stressed the importance of classroom instruction in mental hygiene. In the present article I have ignored the need of this work and have pointed out the necessity of personal supervision of the individual student. It might seem that in doing this I have been inconsistent. Such, however, is not the case. Classroom instruction and personal supervision of the student are both essential parts of a mental hygiene program in a school or college. They are two different avenues of approach, both of which must be used in attacking the problems of mental health, although sometimes we must depend chiefly upon one and sometimes upon the other.

In a preparatory school it would be impossible to accomplish much by means of classroom instruction. The students are too young and lack the background which it is necessary to have in order to understand and profit by such instruction. However, in such a school, a scheme of personal supervision would be comparatively easy to put into operation. The boys are already living in dormitories under the supervision of proctors or housemasters and all that would be necessary in order to put a plan into operation such as that which I have suggested would be to organize these proctors under the direction of the school psychiatrist.

In a graduate or professional school, on the other hand, where one is dealing with a much older group of students accustomed to regulating their own affairs, the case is just the reverse. It would be difficult if not impossible to maintain any degree of personal supervision over such mature students. At the same time, by reason of their mental maturity, they would be able to profit by classroom courses in mental hygiene and would be almost certain to find them interesting.

In a preparatory school, then, mental hygiene should be mainly a program of personal supervision such as I have here outlined, while in a graduate or professional school, the emphasis should be laid chiefly on classroom instruction and the conference work that would grow out of it.

The liberal college lies between these two extremes. The students here are old enough and know enough to enable them to profit by classroom instruction in the principles of mental hygiene. They should receive such instruction as part of their

education. At the same time, as I have endeavored to show, they are in urgent need of personal supervision and such supervision could and should be maintained. Classroom instruction and a scheme of personal supervision are both necessary. The only question would be the purely practical one as to which we should introduce first. In most cases, it seems to me, it would be much easier to begin with the teaching end of the program. The psychiatrist undertaking to develop mental hygiene work in a college can begin by giving courses of instruction in the principles of mental hygiene and doing the conference work that is bound to develop as the result of giving such courses. For the college this is merely a matter of taking on a new professor or instructor and adding one or two new courses to the curriculum. It does not involve any serious modification of the existing organization. This teaching and conference work is very valuable in itself and in it the psychiatrist can easily find enough to do to keep him extremely busy. Later on, when this work is well under way, the other half of the program can be developed.

GROUP TESTS OF PSYCHOPATHIC TENDENCIES IN CHILDREN

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TWO of the leading tests which have been advanced for the detection of psychopathic tendencies in adults are the Woodworth psychoneurotic questionnaire and the Kent-Rosanoff free association test. Both of these tests have been adapted for use with children, the former through the modifications introduced by Mathews[1] and by Cady,[2] and the latter through the compilation of association frequency tables for children.[3] The purpose of the present study was to obtain statistical evidence concerning the value of these two tests—particularly of the free association test—in the detection of children with psychopathic tendencies. Since the psychoneurotic questionnaire and the free association test have been advanced as instruments affording measures of much the same thing, a study was made, also, both of the amount of agreement between the scores yielded by these two tests and of the advantage to be obtained from using them in combination.

The subjects tested were the pupils in four sixth grade classes of the Minneapolis public schools. The total number of children was 162, of which 83 were girls and 79 were boys. Their ages ranged from 9 yrs. 11 mos. to 15 yrs. 5 mos. The average age was 12.29 yrs., P.E.±.69 yrs. With the exception of one negro, all were white. The children were given three group tests: the psychoneurotic questionnaire, the free association test, and the National Intelligence Test, Form A. In addition, each child was rated by his teacher and by the school principal on a rating scale. The questionnaire was identical with the one given in Terman's "Genetic Studies of Genius".[4] The list of words used in the free

[1] Mathews, E., A Study of Emotional Stability in Children, *J. of Delinq.*, **8**, 1923, 1–10.

[2] Cady, V. M., The Estimation of Juvenile Incorrigibility, *J. of Delinq.*, Monog. No. 2.

[3] Woodrow, H., and Lowell, F., Children's Association Frequency Tables, *Psychol. Monog.*, 1916, **22**, No. 5, pp. 110.

[4] Terman, L. M., "Genetic Studies of Genius," Vol. I, 1925, 502–505.

association test was the list for which frequency tables have been published by Woodrow and Lowell. The rating scale consisted of a horizontal line with numbers 1 to 5 set evenly spaced upon it, and was labelled "A scale for rating normality of behavior". The rater placed a check on the line at whatever point he thought best represented the status of the child rated. Descriptive phrases accompanying the scale by which it was attempted to indicate the meaning of the numbers, were as follows:

(1) A definite behavior problem or mentally peculiar.

(2) Unstable, unreliable or showing oddities or peculiarities.

(3) Showing occasional or only slight irregularities of conduct or mental activity.

(4) Possessing good mental attitudes and well behaved.

(5) Unusually healthy-minded, always well behaved; characterized by a desirable mental attitude toward all situations.

The correlation obtained in each of the four schools between the teacher's and the principal's ratings were as follows: School A, +.61; B, +.85; C, +.82; D, +.81. The average for the four schools was +.77. These correlations indicate a reasonable degree of agreement between the teachers and the principals.

Inasmuch as the free association test was given as a group test, the children wrote their responses. The instructions were substantially the same as those used by Woodrow and Lowell, whose frequency tables, also based on written responses, were used in scoring the test. The instructions were repeated in varying phraseology until it seemed clear that the children understood that they were to write the very first word they thought of upon hearing the stimulus-word. The children were told not to hesitate to write the word thought of even though they could not spell it correctly. The test was given in two half-hour periods, 50 words at each period.

It is quite possible that the free association test is most effective when responses are given orally. On the other hand, its greatest value may be to call attention to children who would otherwise be overlooked and who should be given thorough individual examination. For such use, only a group test is likely to prove sufficiently economical to justify itself. It has, moreover, not yet been shown that scores obtained from a written test differ greatly from those of an oral test. Woodrow and Lowell found a high degree of agreement between frequency tables compiled for a group of 1,000 children tested orally and another group of 1,000 who

took the test as a written group test.[5] Moreover, 155 men and women university students showed no reliable difference between the number of individual associations obtained from oral tests scored by reference to the Kent-Rosanoff tables and the number obtained from the same group about six weeks later by a written test.[6]

The childrens' responses to the free association test were scored in a number of different ways, in order to determine what method of scoring yielded the highest correlation with the ratings. The following seven scores were obtained and used in the calculation of correlations with the ratings: the average frequency, as indicated by the Woodrow-Lowell frequency tables, of a subject's one hundred responses; the percentage of his responses which were "favorite" words, a favorite word being the response occurring with the highest frequency in the frequency table for the corresponding stimulus-word; the percentage of the responses which were words that, according to the frequency tables, constitute the three most frequent responses to the corresponding stimulus-word; the percentage of "individual" responses, *i.e.*, those of frequency 0; the percentage of responses of frequency 0 or 1; of frequency 0, 1 or 2; and of frequency 0 to 10 inclusive.

Certain of the free association scores correlated highly with each other. Thus, the correlation between average frequency and the percentage of responses composed of the three response-words of highest table-frequency was +.93. Scores in terms of the number of individual responses, however, showed a correlation with scores in terms of average frequency of only —.63, and with scores in terms of the percentage of responses consisting of the three favorite responses of only —.66.

In the calculation of correlations between ratings and test scores, deviations from the average were labelled positive or negative according to their supposed diagnostic significance. As a result, a positive correlation with the ratings means a positive correlation between goodness or highness of rating and goodness of test score. Goodness of test score, however, means lowness of score in the case of scores in terms of individual reactions or reactions of low frequency, and the Woodworth-Cady scores, while it means highness of score in the case of scores in terms of the average frequency of reaction, the percentage of reactions of

[5] *Op. cit.*, Ch. IX, pp. 105–108.

[6] Clousing, Josephine, Master's Thesis, 1927, under direction of Professor D. G. Paterson, on file in the library of the University of Minnesota. The average number of individual responses by the oral method was 8.53 and by the written method, 9.42.

highest frequency, or the percentage of reactions consisting of words with the three highest frequencies.

The correlations obtained between any of the seven free association scores and an amalgamation of the teachers' and principals' ratings on normality of behavior were low. They varied from one sixth grade to another from —.13 to +.56. The averages of the correlations obtained with the four sixth grades varied with the method of scoring from +.14 to +.29, the highest average correlation, +.29, being obtained when the tests were scored for the percentage of individual responses, *i.e.*, the percentage of each individual's responses consisting of responses not listed in the frequency tables for the corresponding stimulus-words. The average correlations between the amalgamated ratings on "normality of behavior" and the various free association scores were as follows:

Average frequency	+.14
Number of "most frequent" responses	+.14
Number of "three most frequent" responses	+.18
Number of responses of frequency 0–10 inclusive	+.14
Number of responses of frequency 0–2 inclusive	+.19
Number of responses of frequency 0–1 inclusive	+.24
Number of individual responses	+.29

Such low correlations as these afford no indication that the tests may be of value in selecting children who, according to their teachers' judgments, tend toward abnormality of behavior. Upon further examination of the individual responses, however, it was discovered that this heading included reactions which differed so widely one from another that they could not logically be grouped in the same class. It appeared that about one-half of the words which according to the frequency tables were individual reactions were yet natural associations. It was clear that the frequency tables, though based on the responses of 1,000 children, fell far short of including all normal associations.

In the case of some of the stimulus-words the inadequacy of the frequency tables is due to a change in social conditions since the year 1916, when the frequency tables were published. An illustration of this fact is the response "moonshine" to the stimulus-word *whiskey*. This was the commonest response in all the schools, but since the frequency tables were compiled before the passage of the Volstead act, and the word *moonshine* had not at that time been incorporated into the active vocabulary of school children, it had to be counted as an individual association. Other frequently obtained responses indicative of changed social conditions

were the following: *speaker* and *radio* in response to *loud; saxaphone* to *music; scout* to *boy, girl* and *eagle* (the name of the most coveted rank in the Boy Scouts' organization); *furnace* to *stove;* and *bar, O'Henry* and *Baby Ruth* to *candy.*

For the most part, however, responses which seemed normal but were not in the frequency tables served merely to reveal the limitations of the tables. Illustrations of these responses will be given in connection with the scoring rules mentioned below. It is probable that a frequency table, if it is to include practically all normal responses, should be based on at least 10,000 subjects instead of 1,000. It would then include, however, as indeed does one based on 1,000 subjects, a very large number of responses which should be classified as "pathological". It follows that no frequency table can ever provide a strictly objective method for determining whether a response is "normal". Kent and Rosanoff sought to overcome the difficulty by formulating a set of rules, or definitions. Two of these rules are general, that is, apply to all stimulus-words; but there is also a separate set of rules for each of the 100 stimulus-words, so that the total number of rules is no less than 497. Since the frequency tables used in the present study were compiled not with the purpose of devising a test, but with the purpose of comparing the association processes of children with those of adults, they are not accompanied by supplementing rules. Such rules are needed, however, if one is to use these tables in order to distinguish normal from peculiar, or pathological responses. That it is desirable to make this distinction is indicated by correlations between the ratings and the different methods of scoring. These correlations increase with the degree of uncommonness of the responses included in the score. Thus, when the maximal table frequency of the responses counted in scoring is 10, the correlation is +.14; when 2, +.19; when 1, +.24; and when 0, +.29. Since the correlations increase with decrease in the table-frequency of the responses included in the score, it seems desirable to obtain scores consisting of the number of those responses which not only are individual, according to the frequency tables, but which also are so peculiar that their absence from the frequency tables cannot reasonably be assumed to be due simply to the inadequacy of the tables.

For distinguishing between those individual responses which are normal and those which are pathological, a set of 20 rather complex rules was devised. An analysis of these rules revealed the fact that they are more than adequately covered by the rules

of Kent and Rosanoff, provided that allowance is made for changes in social conditions and laws, modern advertising campaigns and modern inventions and innovations of various sorts. Many of the Kent-Rosanoff rules had no application to the responses obtained in the present study, but might conceivably be needed in scoring responses obtained from other children. It is also true that a large proportion of the Kent-Rosanoff rules could be summed up by a small number of more general rules. Just one rule of the proposed list of 20 appeared to cover the great majority of the 497 rules of Kent and Rosanoff. This rule stated that a response should be counted as normal if it consisted of a word of the same category as other words appearing in the frequency table for the corresponding stimulus-word. This rule required for its interpretation a list of illustrations. It meant that the response was to be counted as normal, when, *e.g.*, to the word *sickness,* any disease was mentioned (a number of diseases being already listed in the frequency table); to the word *music,* any common musical instrument (including *saxaphone*); to *fruit,* any fruit; to *stork* or to *eagle,* any bird; to *fun,* any game or variety of play; to *lion,* any animal found in menageries; to *window, sky* (regarded as coördinate with *trees* and *sun,* words occurring in the frequency table for *window*). This same rule meant that if names of any class of objects, such as people, streets, rivers, mountains, nations, oceans or kings, occurred in the frequency table for a given stimulus-word, a response consisting of any other name of the same class of objects should be counted as a normal response to that stimulus-word. Thus, since the frequency table for the stimulus-word *girl* contains the names, *Marian* and *Margaret,* any other feminine first name is counted normal. Since the Kent-Rosanoff rules are already available, and since any condensation of them or special adaptation of them for use with children must needs be tentative until the accumulation of further data, it was decided provisionally to adopt these rules for use in connection with the children's frequency tables. For the 10 stimulus-words in the children's list compiled by Woodrow and Lowell which were not used by Kent and Rosanoff, the 20 rules first considered were used, these rules being interpreted in the spirit of the Kent-Rosanoff rules for the other 90 words. Since anyone familiar with the Kent-Rosanoff rules can easily devise similar ones for use with frequency tables of other words, and since the list of rules used in connection with the 10 words not in the Kent-Rosanoff list is regarded as tentative, it need not

here be detailed. It should be mentioned, however, that a response consisting of the repetition of the stimulus-word or of a variant thereof—a response which cannot be rejected when the test is given as a group test—was ruled a pathological response. The scores which were obtained through the use of the Kent-Rosanoff rules, together with the supplemental list of 20 (this latter including rules for taking into account the effect of recent social changes, as illustrated above) will hereafter be termed "pathological association scores". This term will be used solely to designate scores obtained in the manner described, and without further implications.

To illustrate how greatly pathological association scores differ from individual association scores, the following lists of the individual responses of two subjects are checked as normal (n) or pathological (p).

Subject A	
Individual associations, 14.	
Pathological associations, 6.	
soft—hair	(n)
fun—monkey	(p)
smooth—icy	(n)
candy—earnest	(p)
white—noise	(p)
window—sky	(n)
working—idle	(n)
eagle—scout	(n)
boy—Arnold	(n)
laugh—holler	(n)
child—mouth	(p)
doctor—king	(p)
green—cat	(p)
street—highway	(n)

Subject B	
Individual associations, 14.	
Pathological associations, 11.	
short—same	(p)
beautiful—ragged	(p)
foot—laugh	(p)
anger—laughter	(n)
working—struggling	(n)
sour—poor	(p)
cabbage—cabin	(p)
yellow—scared	(p)
flag—hats off	(p)
lion—male	(p)
square—squar	(p)
loud—danger	(p)
thief—jewel	(n)
moon—stay	(p)

It will be seen from the two illustrations that scoring the responses in terms of the number of pathological associations is an entirely different thing from scoring them in terms of the number of reactions which are simply individual. For the entire group of children, the average number of individual responses was 10.2, $\sigma_{dis.}$ 4.9, while the average number of pathological responses was 4.4, $\sigma_{dis.}$ 2.7. The distribution of both sets of scores was skewed, with the modal score smaller than the average. The average number of individual responses given by the present subjects is much higher than that given by the 1000 subjects tested by Woodrow and Lowell (10.2 *vs.* 5.2). This fact is probably in part a reflection of changed social conditions, the effects of which upon association have already been pointed out. The responses scored as individual included 127 different words which occurred more than once and which, therefore, would not be individual

according to frequency tables based on the responses of the present group of 162 children. It is highly probable, moreover, that many individual responses given only once by the present group would have been obtained again if the group were increased to 1,000 and would thus no longer be individual if scored by reference to frequency tables compiled from responses of 1,000 children similar to those used in the present study. It is quite possible, too, that the comparatively large number of individual associations obtained with the present group has its explanation in part in the fact that three of the four schools from which the subjects were chosen were situated in rather inferior social neighborhoods. In this connection it may be noted that the children of the present group made scores in the Woodworth-Cady test considerably worse than those reported by Terman.[7]

Pathological association scores are much less closely related to the various other association scores obtainable by use of the frequency tables than are individual association scores. Thus, the coefficient of correlation shown by pathological scores with scores in terms of average frequency of response is only —.30, and with scores in terms of the percentage of the three commonest responses, —.33, while the corresponding coefficients for individual association scores are —.63 and —.66.

The reliability of the pathological response scores as determined by the coefficient of correlation between the scores for odd and even numbered responses is +.76, so that the reliability of the entire test, as calculated by the Brown-Spearman formula, is +.86. To determine the objectivity of the pathological association scores the papers were scored independently by the writers, with the result that the two sets of pathological association scores showed a correlation for the entire group of 162 subjects of +.94.

The scores in terms of pathological reactions yielded an average correlation with the amalgamated ratings which was decidedly higher than that obtained with the scores in terms of individual reactions, namely +.57 as compared with +.29. The average of the coefficients of correlation between the pathological association scores and the crude National Intelligence scores was +.30, and the average of the coefficients of partial correlation between the pathological association scores and the amalgamated ratings, with the intelligence scores constant, was +.48. The correlations obtained with the pathological association scores and their probable errors, are given in Table I.

[7] "Genetic Studies of Genius," Vol. I, 1925, p. 509.

TABLE I

COEFFICIENTS OF CORRELATIONS OBTAINED WITH "PATHOLOGICAL" ASSOCIATION SCORES
The table gives Pearson coefficients (r) with their probable errors.

Score with which correlated.	School				
	I	II	III	IV	Av.
Amalgamated Ratings	+.63 ±.07	+.61 ±.06	+.37 ±.09	+.67 ±.06	+.57
Amalgamated Ratings (with Nat'l Intell. Score constant)	+.48 ±.09	+.42 ±.08	+.37 ±.09	+.65 ±.07	+.48
Woodworth-Cady	+.37 ±.10	.00 ±.09	+.25 ±.10	+.17 ±.11	+.20
Nat'l Intelligence	+.26 ±.10	+.50 ±.07	+.06 ±.11	+.37 ±.10	+.30
Chronological Age	—.26 ±.10	—.07 ±.09	—.21 ±.10	—.74 ±.05	—.32

It will be observed from Table I that the number of pathological associations is smaller (*i.e.*, the score is better) in those of higher intelligence (av. r = +.30) and also in those of lower CA (av. r = —.32). This apparent contradiction is due to the fact that the correlation between CA and MA is negative, as is often the case in children enrolled in a given school grade. For the 162 cases taken as one group, the following correlations, r's, with their P.E.'s, were obtained:

Pathological scores and intelligence scores	+.28±.05
Pathological scores and IQ	+.31±.05
Pathological scores and CA	—.24±.05
Intelligence scores and CA	—.27±.05

The correlation between the infrequency of individual association scores (number of responses of zero frequency) and intelligence scores was also calculated, for the 162 children taken as one group, and found to be only +.15 ±.05.

In determining the pathological association scores, omissions of responses were ignored. Omissions occurred in 72, or 0.45 per cent. of the 16,200 responses that would have been obtained had there been no omissions. These omissions were not found to have diagnostic significance, since, when scores consisting of the number of pathological associations plus the number of omissions are correlated with the ratings, the coefficients are slightly lower, on the average, than when pathological scores alone are used. For the four schools, respectively, the coefficients obtained with omissions included are as follows: +.55, +.65, +.32 and +.56.

The average of these coefficients is +.52, whereas the average of the corresponding coefficients when pathological scores alone are used is +.57.

The average and variability ($\sigma_{dis.}$) of the scores used in calculating the correlations shown in Table I, are given, for each of the four grades and for the group as a whole, in Table II.

TABLE II

AVERAGES AND VARIABILITIES OF EACH GROUP OF SUBJECTS IN THE SCORES CORRELATED

No. of children: school I, 37; school II, 51; school III, 39; school IV, 35; total, 162.

Score	School I	School II	School III	School IV	Total Group
Chronological Age	11.87 yrs. ± .92	12.55 yrs. ± 1.00	12.39 yrs. ± 1.28	12.26 yrs. ± 1.33	12.29 yrs. ± 1.02
Nat'l. Intelligence	111.5 ±18.7	106.1 ±18.5	107.0 ±16.8	98.8 ±19.2	106.0 ±18.8
Woodworth-Cady	17.7 ± 9.3	17.7 ± 7.4	20.8 ± 9.2	22.1 ±10.8	19.4 ± 9.0
Pathological Ass'n	4.5 ± 2.5	4.4 ± 2.2	4.0 ± 2.4	4.6 ± 3.5	4.4 ± 2.7
Amalgamated Ratings	3.3 ± .97	3.0 ± 1.37	3.1 ± 1.02	3.4 ± 1.04	3.2 ± 1.15

The correlations between the Woodworth-Cady scores and amalgamated ratings averaged only +.25. All the correlations obtained with the Woodworth-Cady test, together with their probable errors, are shown in Table III.

TABLE III

COEFFICIENTS OF CORRELATION OBTAINED WITH WOODWORTH-CADY SCORES

Score with which correlated.	School I	School II	School III	School IV	Av.
Amalgamated Ratings	+.49 ±.09	+.07 ±.09	+.19 ±.10	+.26 ±.11	+.25
Amalgamated Ratings (with Nat'l Intell. Score Constant)	+.46 ±.09	+.03 ±.09	+.31 ±.10	+.28 ±.11	+.25
Nat'l Intelligence	+.19 ±.11	+.08 ±.09	—.12 ±.11	+.01 ±.11	+.04
Pathological Ass'n	+.37 ±.10	.00 ±.09	+.25 ±.10	+.17 ±.11	+.20

The hope was originally entertained that an amalgamation of Woodworth-Cady scores with free association scores would show a correlation with the ratings higher than that obtained with

the scores of either test taken alone. Because of the low correlation, however, between the Woodworth-Cady scores and the ratings (averaging +.25), not much benefit results from such an amalgamation. The multiple correlations calculated between the ratings and an amalgamation of the scores of the two tests gave for the four schools, respectively, the following coefficients: +.85, +.61, +.42, and +.73. The average of these coefficients of multiple correlation is +.65, which is 8 points higher than the corresponding coefficient of correlation between ratings and pathological association scores, the latter being +.57.

There appeared to be no way to revise the Woodworth-Cady scores so as to raise the correlations. It was found, however, that there were 15 questions in the Woodworth-Cady list which failed to show any larger number of diagnostic answers from that 10 per cent of the group who were rated lowest, or most psychopathic, than from that 10 per cent who were rated highest. Scoring the test with these 15 questions omitted resulted simply in lowering the average of the correlations with the ratings from +.25 to +.24.

That the ratings of the teachers and principals, even when amalgamated, are far from being entirely valid indices of psychopathy as defined by the rating scale, is hardly open to question. While it is probable that the raters comprehended rather well what was meant by the descriptive phrases of the rating scale, they would nevertheless be unable to make errorless ratings because of inadequate means of appraising differences between the children. It seems very probable, moreover, that one large source of error would lie in the inability to appraise a child's tendency towards psychopathy apart from his intelligence. The teacher's ratings are inevitably based in large part upon observations of pupils in the situation of endeavoring to learn. The ratings of any child, consequently, would doubtless be biased by what is largely a matter of intelligence, that is, the child's ease of learning. Evidence in support of this hypothesis lies in the fact that the ratings correlate considerably higher with an amalgamation of free association and intelligence scores than with the scores of either of these tests taken alone. The coefficient of multiple correlation between the ratings and an amalgamation of pathological association scores with National Intelligence Test scores (calculated from the average correlations shown by the four school grades) is +.73. In other words, scores based on a combination of these two tests agree with the ratings almost as well as the ratings of the teachers agree with those of the principals. It

appears, then, that the ratings were ratings of something which, while measured better by the free association test than by the intelligence test, is measured best by a combination of the two tests.

As regards the practical use of the tests in detecting children with psychopathic tendencies, the above findings do not indicate that in the case of sixth grade children either the Woodworth-Cady questionnaire or the free association test scored in terms of the number of individual associations is of any great value. On the other hand, the correlations between the ratings and the free association test scored in terms of the number of pathological associations are high, in view of the unavoidable errors in the ratings, and are even higher with a combination of pathological scores and Woodworth-Cady scores; and they would appear to justify the use of the association test, either alone or in conjunction with the Woodworth-Cady test, to pick out children who should be given especial observation as regards abnormality of mental development. It is probably conservative to say that such individual study should be made of all children whose pathological associations exceed the average number by twice the standard deviation of the group, that is, by those children giving 10 or more pathological associations. Such children comprised 6.1 per cent of the present group.

GENERAL CONCLUSIONS

Among the findings of the present study, the following appear to merit especial emphasis.

(1) Various scores obtainable from the free association test differ considerably in the degree of correlation existing between them. For example, scores in terms of average frequency showed the following correlations: with the number of times a child responded with one of the three commonest responses to the corresponding stimulus-word, +.93; with the number of "individual" responses, —.63; and with the number of "pathological" responses, —.30.

(2) No existing frequency table suffices for the determination of peculiar or "pathological" responses in a purely objective manner. If such determinations are desired, the frequency tables must be supplemented by a set of rules.

(3) Of the various scores obtainable from the free association test, the one which agrees best with ratings on "normality of behavior" is that representing the number of pathological re-

sponses. In the present study such scores yielded a coefficient of correlation about twice as large as did scores consisting of the number of merely individual responses, *i.e.*, responses not occurring in the frequency tables.

(4) Pathological association scores were found to give an average correlation of +.57 with amalgamated ratings of normality of behavior, those with inferior ratings tending to give the larger number of pathological associations. Such pathological scores were found, further, to be fairly reliable (Brown-Spearman coefficient, +.86).

(5) Infrequency of pathological associations, that is, goodness of score, was found to show a very slight, but reliably positive, correlation with intelligence (+.28 ±.05) and with brightness as measured by IQ (+.31 ±.05). The correlation between infrequency of "individual" associations and intelligence was too small to be significant (+.15 ±.05).

(6) Woodworth-Cady scores were found to show a correlation averaging only +.25 with the ratings, and only +.20 with pathological association scores. They show practically no correlation with intelligence (r, +.04).

(7) An amalgamation of Woodworth-Cady scores and pathological association scores would show considerable agreement with the ratings and more than do the pathological scores alone (multiple coefficient of correlation, +.65).

(8) The ratings by teachers and principals upon a rating scale of "normality of behavior", which were used in this study as a criterion of tendency towards psychopathy, were probably unduly biased by differences between the children in intelligence, since the multiple correlation between the ratings and an amalgamation of intelligence and pathological association scores was +.73. The partial correlation, however, between pathological association scores and the ratings, with intelligence constant, was +.48.

DO GROUPS THINK MORE EFFICIENTLY THAN INDIVIDUALS?

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IT is readily demonstrated that faith in the practice of group thinking is not uniform. In some business corporations committee action is the rule, in others one good man is selected and made responsible. College students are sometimes set to work upon individual term papers, sometimes upon group projects. Research is usually an individual task, occasionally a group product.

Opinions upon the value of the group unit for intellectual tasks are varied. Some persons believe that they create a better product working by themselves. They lay emphasis upon the self-consciousness, confusion, distractions, interruptions, and irrelevant personality conflicts which arise in groups. Other persons prefer to work as one of a group. They lay emphasis upon the way in which the group stimulates the individual to do his best, suggests to him lines of thought he might otherwise have missed or rejected and corrects the errors of individual bias. Some recent developments in educational and political theory have laid great stress upon the need for group activities. Miss Follett,[1] in "The New State" elaborates an appealing theory of the integration of individual contributions in a group-thinking process. Overstreet[2] writes, "In every coming together of minds that are serious in the effort to understand, there is something more than the minds. There is the Creative Plus, which no one mind by itself could achieve." A series of directions for discussion groups contains the statement, "Trust the group. There is no person in it who is not superior to the rest in at least one respect. The experience of all is richer than the experience of any. The group as a whole can see further and more truly than its best member."

Experimental evidence is limited. Thie[3] in connection with high school classes in English, and Barton[4] in the field of algebra

[1] Follett, The New State, Longmans, 1920.
[2] Overstreet, Influencing Human Behavior, People's Institute Publishing Co., 1925.
[3] Thie, The Efficiency of the Group Method, *English Journal*, 1925, 14:134.
[4] Barton, Group Activity versus Individual Effort in Developing Ability to Solve Problems in First Year Algebra, *Journal of Educational Administration and Supervision*, November, 1926, pp. 512–518.

have demonstrated that students working together in small groups progress more satisfactorily in several respects than do students working by more traditional methods. Miss Gordon[5] has shown that the average of individual judgments in such matters as esthetics or in the judgment of a lifted weight is equal or superior to the judgment of the best individual within the group. The larger the number of individual judgments averaged, the more closely does the resulting figure approach the true value. This is encouraging evidence on the quality of the group product.

There remain other questions, among them the question of speed of production. The purpose of the experiment to be reported here was to test the intellectual efficiency of a group as compared with the efficiency of the same individuals working by themselves. The task chosen was that of making up words out of the letters contained in a given word. The subjects were 108 graduate students of education. They were divided into twenty groups varying in size from three to ten. After careful instruction[6] and fifteen minutes of preliminary practice to reduce the handicap of persons who had never done this word-building before, each group was given envelopes containing the first set of basic words, out of the letters of which new words were to be made. Four basic words were chosen, each containing nine letters, four of which were vowels, and one an "s", with no letter appearing more than once in a word. The words were "Educators", "Neurotics", "Secondary" and "Universal". The words were rotated among the groups, during the four periods of the experiment in such a way that no ease or difficulty of a particular word would contribute to one period more than to any other. As a matter of fact, as is shown in Table I, the differences between words were slight, "universal" being somewhat more difficult than the other three.

The first ten-minute period was spent by each person making as many words as he could, by himself. He sat where no other could see his paper, and wrote down words as fast as he could think of them. After a brief intermission, the group came together for a group-thinking process. The group secretary kept the list of words, and each person called his contribution aloud. Ideas, just in the process of becoming suitable words were expressed in

[5] Gordon, K. Group Judgments in the Field of Lifted Weights, *Journal of Experimental Psychology*, Volume III: 1924, pp. 398–400.
Gordon, K. Group Judgments in the Field of Esthetic Judgment, *Journal of Experimental Psychology*, February, 1923, p. 36.

[6] Directions are inserted here for the benefit of groups wishing to repeat the experiment, particularly for classes in experimental psychology.

Directions for Experiment in Group Thinking

1. The group secretary is not to participate in the experiment except by reading instructions, keeping time, and writing the list of words produced during the group work. Secretary should be a rapid writer.

2. Read these directions aloud, to the entire group and make sure everyone understands. Before each section of the experiment read again the pertinent directions.

3. Provide each person with paper and pencil. Sit so that no one can by any chance see another's work. Place your name and a number I at the top of the sheet.

4. Open envelope No. I, and announce the source word, stating and spelling it clearly. Secretary keep time record to the exact second. Others immediately upon the announcement write the source word at the top of their own sheets, and begin trying to make words from the letters.

5. Make as many words as possible, using only the letters in the source word. No letter may be used more often than it appears in the word. No word of less than three letters may be counted. Three letter words are all right. No proper names or abbreviations may be counted. Words made by adding "s" or "es" are not counted as additional words. Other modifications are counted as new words. If in doubt about a word write it down, but place a question mark in front of it for later verification. Try not to waste time over some particular word, or by slow writing.

6. No dictionaries or other books may be used as helps during the experiment.

7. Stop immediately when the secretary calls "stop". This should be ten minutes to the second, after announcing the word. Collect papers immediately and clip them together.

8. Assemble in a group, and take another sheet of paper. Place your name, and a number II on this sheet.

9. Opening envelope No. II and announce the second source word, stating it and spelling it. Note the time exactly. Everyone write the word immediately at the top of his paper.

10. Proceed to make as many words as possible, under the rules outlined in 5. In this case think out loud. Announce your word as you think of it. If you "almost" think of a word, speak out the idea you have. Perhaps someone else can hit upon the exact form. Help one another in the interests of a group product. Everyone should participate as much as he can.

11. The secretary should jot down rapidly every word suggested. Others need not try to keep a record. The paper is only for noting suggestions as they arise. Try not to waste time over any particular suggestion, or by slow writing.

12. At exactly ten minutes from the time of the announcement of the second source word call "stop".

13. After three minutes of relaxation, assemble in a group, open envelope No. III and announce and spell the third source word. This is to be a group effort, just like the second. Make suggestions freely. Let the secretary keep the record.

14. At exactly ten minutes from the time of the announcement of the third source word, call, "stop".

15. Take positions, as at first, so that each may work individually without any danger of seeing the words written by others. Place names on papers. Open envelope No. IV and announce and spell the fourth word. Write it on the paper, and at once begin to construct as many words as possible.

16. After exactly ten minutes, call "stop". Collect papers and clip together.

Directions for Recording Results

1. Go over every list, crossing out illegal words and duplicates. This should be checked by two persons independently.

2. Enter on report blank name of each person, and number of correct words produced in the period when working individually.

3. Find the average number for each period and enter it below.

4. Compile a total list, showing the number of different words thought of by the group during the individual work period. The best way to do this is to start with the longest individual list; let someone read the remaining lists while duplicates are checked and new ones added. Notice how many different words are represented in this composite list.

REPORT BLANK

Committee..... Secretary.....

1st word..... 2nd word..... 3rd word..... 4th word.....

Name	1st Period Individual Score	4th Period Individual Score	2nd Period: Group Score .....
			3rd Period: Group Score .....
Average			
Total Composite			

NOTE: Clip all original papers together, and turn in with this report.
Add on separate sheet any comments or interpretations for the results in your group.

the group, and others helped to supply the desired term or modification. This continued for exactly ten minutes. After a longer period of relaxation the group came together again for a second period of group thinking, opening this time the envelope containing the third basic word. The fourth and final period was like the first one, a period in which each individual constructed as many as he could. Thus practice effect probably contributed about equally to success in the group process and to success in the individual process.

Table II presents the results. The groups averaged five or six persons each. The poorest individual working by himself constructed about 18 words in the ten minutes. The average individual constructed 32 words, while the best in each group constructed 49 words. In the periods of coöperative work, the same persons produced about 75 words, on the average. This, although far more than was produced by the best individual working by himself was slightly less than the group product obtained from a net-total of all words produced by those individuals when they were working separately.

TABLE I

COMPARISONS OF DIFFICULTY OF WORDS USED
(All averages are accompanied by S. D. of average)

Scores Made	Words			
	Educators	Neurotics	Secondary	Universal
Individual: First trial	32±2.9	37±6.3	30±1.4	24±1.4
Individual: Second trial	40±1.8	35±2.2	37±2.7	25±1.2
Groups: First trial	74±6.4	69±3.3	71±1.0	70±5.5
Groups: Second trial	86±2.5	77±4.1	87+5.1	63+3.1
Average	58	55	56	46

TABLE II

COMPARISON OF INDIVIDUAL PRODUCTS WITH GROUP PRODUCTS

Factor	Mean	Standard Deviation	Coefficient of Variation
1. Size of group	5.4	1.9	35.2
2. Words constructed by *poorest* individual of group, working alone	17.8	6.7	37.6
3. Words constructed by *average* individual of group, working alone	32.3	5.8	17.9
4. Words constructed by *best* individual of group, working alone	48.6	9.7	20.0
5. Words constructed by *group*, all co-operating	74.9	10.5	14.0
6. Total of different words constructed by all members of group, working alone	86.8	14.9	17.2

Stated in units of standard deviation of the difference, the average person surpassed the poorest of his group by a difference seven times its S. D.; the best person surpassed the average by a difference six times its S. D.; the group work surpassed the average individual production by a difference 16 times its S. D.; the coöperative group work surpassed the work of the best individual by a difference of 8 times its S. D.; but the group product was less than the group composite of all work done individually, by a difference equal to three times its S. D.

If comparison be made in terms of the average individual production, the poorest person has an efficiency of 55 per cent, the best of 150 per cent, the coöperative group of 231 per cent and the compiled group, using the sum-total of individual products, would average 268 per cent. In every group the group thinking produced more words than did the best individual of the group. In five of the twenty groups the coöperative group work produced more words than did the compilation of a group total list based upon individual activity.

It would seem fair to conclude that in a task of this sort, building words for a ten-minute period, a group of from three to ten persons can be counted upon to do better work than the average

or best person in the group. Only rarely is the group likely to do better work in oral discussion than would be attained if it were possible to add together a sum total of individual contributions.

It may be questioned whether many tasks permit the simple addition of one man's product to that of another. If such situations are few, it is fortunate that the coöperative, oral, group-thinking process is practically as efficient as the group compilation. Further experiment is needed to determine whether the group would prove more or less efficient if the task were to involve a long project with many complex adjustments.

Table II gives also the coefficient of variation. Groups are, as one would expect, more alike than individuals, but coöperative group products vary even less than average ability within the groups. A democracy of group units would seem to be not only above the average ability of the individuals concerned, but also much less diverse. Another interesting fact is the wide variation in ability among the persons making low score for their group. This scattering would seem to be a result of selecting groups high on the ability scale so that the poor ones scatter down over a long range.

The intercorrelations reported in Table III show the relationships among certain factors involved in this experiment. Size of group seems to be related about equally to net-total of individual production (.59) and to group production (.53). Larger size apparently increases the superiority of the group product over the average individual work ($r = .64$). The average intelligence of the small group, as measured by the Otis Higher Examination, and within the range of the graduate students included in the experiment, is not highly related to any factor. The relationship to score of average individual in word building is .46. The scores of poorest and best individuals should, of course, show a relation to average individual production within which they play a considerable part. The correlations are .60 for the relation of poorest to average and .77 for the relation of best to average. It is interesting to note that the variation in *best* individual production was more potent not only in influencing averages but also in influencing the net-total of individual productions. The variations among "poorests" were negligible (.04) in their influence upon the total product, while the variations among "bests" mattered greatly (.61). In measuring the output of a group, either when working along coöperative group-thinking lines, or when the project permits simple compilation of individual efforts, it matters little about the

ability of the poorest or even the average person of the group. The result seems to show primarily what the few ablest in the group have produced.

The eighth item in the intercorrelation table is the score on a committee project carried out by much the same group during the semester course in psychology. The papers turned in by these groups were graded subjectively on an A to F scale (five point)

TABLE III

INTERCORRELATION OF SOME FACTORS RELATED TO WORD PRODUCTION
(Numbers across the top of the columns refer to the factors listed at the left of the table.)

	1	2	3	4	5	6	7	8	9	10
1. Size of group	...	—.45	—.51	—.23	.10	.59	.53	—.10	.27	.64
2. Average Otis Intelligence Score	—.45	...	.30	.46	.49	.07	.09	.38	.00	—.13
3. Score of *poorest* individual	—.51	.30	...	.60	.25	.04	—.19	.03	.13	—.53
4. Score of *average* individual	—.23	.46	.60	...	.77	.51	.16	.05	.42	—.38
5. Score of *best* individual	.10	.49	.25	.77	...	.61	.53	—.52	.26	.10
6. Net total individual production	.59	.07	.04	.51	.61	...	.35	—.02	.76	.07
7. Group production, working in cooperation	.53	.09	—.19	.16	.53	.35	...	.09	—.27	.78
8. Class Committee Report	—.10	.38	.03	.05	—.52	—.02	.09	...	—.11	.06
9. Superiority of individual summation over group product	.27	.00	.18	.42	.26	.76	—.27	—.11	...	—.51
10. Superiority of group product over score of average individual	.64	—.13	—.53	—.38	.10	.07	.78	.06	—.51	...

STANDARD ERRORS

r.		*S.E.*
.00	=	.22
.20	=	.21
.40	=	.19
.60	=	.14
.80	=	.08

and hence the low correlations may not be surprising. Certainly efficiency in group thinking as measured by word-building would have been a poor basis for predicting the quality of committee work to be done, to the extent that that quality was measured by term paper grades.

The ninth line of intercorrelation seeks to discover the factors most potent in causing the group compilation of individual work

to surpass the coöperative group-thinking product. The results show only that when individual summation was high, it tended to surpass the group product and vice versa. The tenth item, the measure of superiority of the coöperative process over the average level of individual work, is equally unrelated to other factors. There are clearly factors making some groups efficient as groups which are not indicated by the size of the group, the average intelligence of the group, or the individual efficiency of the members at the task. One would hazard a guess that among these contributing factors are many which can be improved through practice. Probably willingness to offer ideas, ability to apprehend ideas expressed by others, and tendency to repress irrelevant comments, would be included. This type of group task might be used to test the extent to which certain types of class training supposed to develop ability to work with others really show results in terms of increased group efficiency as compared with the level of individual efficiency.

Table IV shows the operation of a few factors, cleared of the influence of certain encumbering relationships. Thus a chance correlation of —.45 between size of group and average intelligence of the group conceals many of the operations of these factors, except as partial correlations make it possible to study them. The partial correlations suggest that in groups of the same size intelligence is a fairly important factor (.44) in determining group production, more important, surely, than average individual production at the same task (.14).

TABLE IV

SOME PARTIAL CORRELATIONS

With Size of Group Constant:	
Average intelligence, and group production	.44
Average intelligence, and superiority of group product over average individual	.23
Average intelligence, and superiority of net total of all individual work over group production	.14
Average individual production, and average group production	.14
Average individual production, and superiority of net total of individual products over group product	.47
With Average Intelligence and Average Individual Production Constant:	
Size of group, and group production	.65
Size of group, and superiority of net total of individual products over group product	.36
Size of group, and superiority of group product over that of average individual	.69

This is suggestive. What a group produces depends not so much on average individual ability at the specific task in hand, as on average individual intelligence. In other words, group produc-

tion may be largely a result of factors other than the sort of individual training which education has so often sought to give. These other factors which are important in determining how well a group can do are related to intelligence, although, as suggested, presumably dependent in part on practice in group work.

In studying the influence of the factor of size, when intelligence and individual capacity for production have been held constant, it appears that within the limits of size of groups studied (3 to 10 persons) the larger groups produce more words ($r = .65$) although as the groups increase in size there is some tendency for the method of compilation from individual reports to increase in its superiority to the method of coöperative discussion ($r = .36$).

The experiment suggests the following hypotheses, limited, of course, by the simplicity of the behavior examined and the homogeneity of this high ability class.

1. The product of group thinking is distinctly superior to that of the average and even that of the best member of the group. In this case it is further above the best than the best is above the average.

2. In most cases, with so simple a task, a division among individuals and a summation would give a better product than is obtained by coöperative group discussion. This could not, of course, be applied to the learning that takes place. Individual learning may well be greater when one, as a part of the coöperative group, helps to produce a far larger product than one could when working alone.

3. Groups are more alike than random, or even average, individuals chosen from those groups.

4. The larger the group, within the range from three to ten, here studied, the more superior the group product becomes.

5. Variation in group production depends much more upon how able the best members are than it does upon the ability of those further down in the ability scale.

6. Group production is a matter so different from individual production in the same field that one is practically no index of the other. Most of the factors that make for efficient work as a member of the group, lie outside the range of the things we are doing in education to equip individuals to do tasks by themselves.

AN EXPERIMENTAL STUDY OF FUNCTIONAL ANESTHESIAS AS INDUCED BY SUGGESTION IN HYPNOSIS

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1: *Report of the Experiments*[1]

THE experiments, to be reported, were carried out in the new Department of Abnormal Psychology at Harvard University, during a large part of the college year 1926–27 and during the autumn, 1927. Valuable assistance during the experimentation was given by Mrs. H. Lundholm, and by two students of psychology, Miss M. B. Mitchell of Radcliffe and Mr. L. R. Weiss of Harvard. The subjects, all men and volunteers, were not, with the exception of one, students of psychology, and were, as we think, nonbiased by any theory concerning the nature of the experiments.

The main purpose of the experiments has been to try to investigate, by means of a conditioned response method, what is called mental dissociation, whatever the latter may mean. More specifically the problem has been to study artificially induced anesthesias by investigating their influence both on the conditioning of responses and on responses already conditioned.

The very first experiments were determined by a notion that we conceived of as being possible, namely, that a response may be conditioned by medium of a *co-conscious* or *subconscious* nucleus of awareness, which the subject thus during the experimentation is not consciously aware of. Accordingly we began by trying to condition a response by a stimulation for which the subjects had an artificially induced anesthesia. We had devised, a priori, a whole series of further experiments that were going to follow this, but as our devise was based on certain expectations of what was going to happen, and as the happenings did not follow our expectations, we were already in the very beginning sidetracked from our plans and from a strictly laid out method of procedure, and after this the work has been more like the exploring of a new land, and the route has been full of excitements and surprises.

[1] This report was read at the Psychological Clinic of Harvard University, on January 13, 1928.

However, we have tried and, as we think, partly succeeded in tracing some psychological principles connecting the phenomena, as they have been directly given by the experiments.

We have been working with four men subjects, all of whom we could easily bring into a deep state of hypnosis, and all of whom showed complete post-hypnotic amnesia for any suggestion given during the hypnotic sleep. The results from two of the subjects have all through been similar, and, hence, we need in the coming report only to follow one of these. The phenomena connected with the third and fourth subjects, on the other hand, present deviations that will be treated separately. It should be noticed that the different moves of the experiments to be described, all cover large numbers of series of trials, generally undertaken on several consecutive days. A series of trials always contained 100 stimulations, out of which 80 were stimulations with both adequate and nonadequate stimuli, and 20 (irregularly spread through the series) stimulations with only the nonadequate stimulus. In the first part of the experiments we used electrical shock as the adequate stimulus for a withdrawal response of the right middle finger from the electrical contact, a sound-click, produced by the ordinary telegraphic sounder as nonadequate stimulus for the same response. In the latter part we repeated certain features of the experiments, using electric shock and the flashing up of a small electrical lamp as stimuli, and added a few new features with this laboratory device.

In accordance with our plan, we started the experiments with Mr. V. by trying to build up a conditioned response to a stimulation for which the subject during the conditioning had an artificially induced anesthesia. The experiments were carried out with the subject in a post-hypnotic, fully waking condition, but in which he was deaf for the sound-click, the deafness being due to preceding suggestion during hypnotic sleep. The result of this attempt was entirely negative. Our preconceived notion that a conditioned response possibly might be built up by medium of a coconscious fact of awareness proved to be wrong. After 12 series of experimentation, containing altogether not less than 1200 trials, there was not the vaguest indication of a tendency to withdraw the finger from the electrical contact when the click was sounded without the shock. In the normal individual this withdrawal becomes a fairly well established habit after only 50 trials or less. In fact, the conditioned response we have chosen to work with is one of the most easily established of all.

Our next move was now to build up the conditioned response in fully normal condition, when the subject was clearly aware of both the adequate and the nonadequate stimulations. The first series of trials seemed to indicate a tendency of establishment of the conditioned response but as we continued, this tendency entirely disappeared and we arrived at a point when there was not a single withdrawal on click without shock. This was rather surprising and we asked ourselves how it might have come about. The answer that first occurred to us was that perhaps the subject, for some or other reason, might have built up an attitude of consciously wishing to resist the impulse of withdrawal when the sound alone was given. We made a few series of experiments on normal control subjects, showing that if they are instructed to try to resist or inhibit the withdrawal, except when they feel the shock, they may partly succeed in doing so, however not nearly as well as our subject V. While in this attitude the control subjects were noticed to show a facial expression of utmost attention to the performance of the experiment all through the series, while Mr. V. showed a rather blank and noninterested facial expression all the time. A further line of reasoning was this: If our subject V. consciously attempts to inhibit impulses to withdraw his finger when he is stimulated only by the sound, distraction of his attention during the performance ought to interfere with this inhibition. Consequently, we carried him through two series of experiments, during which his attention was distracted by mathematical calculation. We had him, while he was doing the reactions, subtract successively 7 from 100, etc., and later add, subtract, multiply and divide numbers read to him by one of the assistant experimentors. Even under such conditions there was not one single withdrawal except when the shock was given. Still another trick was tried to see if we could interfere with the inhibition. We instructed the subject to pay attention to the sound, not to his finger, during the experiment. We suspected that the subject previously perhaps had paid strong attention to the finger, and that this might have resulted in the successful inhibition. As before there was no withdrawal except on the shock.

By and by the thought came to our mind that this inhibition—we could not possibly rid ourselves from the view that there had been some kind of inhibition at work—perhaps was not at all consciously willed, but may have been due to some or other coconscious activity, and we could theoretically conceive of the possibility that such an action tendency perhaps had been

strongly established during the first phase of the experiment, when the subject was in the post-hypnotic condition and deaf for the nonadequate stimulus. Although in these experiments the only thing suggested to the subject had been post-hypnotic anesthesia for a certain sound, it may be that this suggestion had associated with it a positive tendency of inhibition, relative to withdrawal on the stimulation that was not consciously perceived. Furthermore this inhibitory tendency may have been so strongly established during the experiments, referred to, that it outlasted the conditions of the second phase of experimentation, still influencing the reactions of the subject in apparently normal condition. If this working hypothesis were true it seemed to us that a contra-suggestion, given to the subject in hypnosis, ought to do away with the inhibition. Accordingly we hypnotized the subject and gave him the following suggestion: "When you wake up you will not resist any impulse to withdraw your finger from the electrical contact, not even the vaguest impulses." Then we woke him up and tried him again. Just as before, not a single withdrawal on sound without shock. The inhibitory tendency, whatever it may be, was equally effective after the contra-suggestion.

We were puzzled and quite discouraged, and tried all kinds of reasonings to explain our failure to break down the inhibitory impulse. Finally we made one more attempt, this determined by the following consideration: perhaps our contra-suggestion had not reached the hypnosis integrate of personality that was inclusive of the inhibitory tendency. We hypnotized the subject once more and this time we investigated the content of his mind during the sleep by questioning him concerning the experiments. And now it was shown, that first we obtained an integrate that remembered having heard the click, but that was neither aware of having felt any impulse of withdrawal when the click was heard without the shock given, nor of having ever inhibited or resisted any impulse to withdraw the finger from the contact during the experiment. Only by bringing the subject repeatedly into deeper and deeper sleep did we finally reach another integrate, that admitted both having heard the click, having felt an impulse to withdraw on click without shock and having resisted and inhibited that impulse. To this all inclusive integrate we repeated the contra-suggestion. And now we got, when we again continued the experiment through two series of trials, 26 withdrawal responses to sound without shock, out of 40 stimulations by sound without shock, showing that this time the contra-suggestion had been effective. At

the following session of experimentation, two days later, the effect of the contra-suggestion was still there. We got 70 withdrawals on sound without shock out of 200 stimulation with sound without shock. Besides that we get a new kind of incomplete withdrawal-responses on sound without shock, that by the observer was described as follows: "This response is not a withdrawal. The finger never leaves the contact, but there is a definite jerk of the hand in these instances." Seventy-four such responses occurred in addition to the 70 completely positive responses in the 200 trials with sound without shock. We had already at the previous session ventured to interpret the existence of both withdrawal responses and absence of such when the sound alone was given as indicative of a co-conscious conflict or battle between an inhibitory tendency and another tendency set up by the contra-suggestion, a battle in which sometimes one, sometimes the other tendency was the victor. Now we felt that this new type of half responses might be expressive of the same conflict. When we continued the experiments again two days later the effect of the contra-suggestion was diminished and seemed to gradually fade away. It was however still manifested in 35 positive responses and 35 half responses out of 120 trials with nonadequate stimulus alone.

The next move of the experiment was determined by the following consideration: if the performance we had seen during the last three days of experimentation was due to a co-conscious conflict, we ought to be able to solve this conflict by strengthening in turn one or the other of the conflicting tendencies. The solving of the conflict in favor of the tendency of the contra-suggestion would theoretically be brought about by repeating forcefully the contra-suggestion in hypnosis, the solution of the conflict in favor of the inhibitory tendency would be brought about by letting the subject again go through a number of series of trials in a post-hypnotic condition with deafness for the non-adequate stimulus. As you remember, we had assumed that this inhibitory tendency originally had been built up during the first phase of the experiment, when the subject in post-hypnotic condition was deaf to the sound-click.

In accordance with this working hypothesis we hypnotized the subject and gave again the contra-suggestion. The experiments then carried out showed withdrawal response every single time the nonadequate stimulus was given alone. Then we hypnotized the subject again giving him the suggestion that when he woke up he would not hear any other sound than the hypnotizer's voice.

We woke him up, assured ourselves that in the post-hypnotic condition he really was deaf for the sound-click, and continued the experiment. Now the withdrawal responses on the nonadequate stimulus alone was entirely gone. Not a single one occurred. Finally we brought the subject back in normal condition, when he again heard the sound, and now we got back the typical conflict series. There were 12 withdrawal responses, 5 half withdrawals, and 3 negative responses in a series where altogether 20 stimulations with sound without shock occurred. Our last experiments had thus apparently shown that we could switch strength to any one of the conflicting tendencies by repeating the contra-suggestion on the one hand and by carrying on experiments in post-hypnotic condition with anesthesia for the nonadequate stimulation, on the other. We succeeded fully in solving the conflict by thus favoring the tendency of the contra-suggestion, but did not succeed fully in doing the same by favoring the inhibitory tendency.

When we had conducted the experiments with sound and shock so far we decided to try to repeat them with light and shock. We brought the subject into a post-hypnotic condition in which he was blind for the electrical lamp, the lighting up of which was the nonadequate stimulus in these series. As before we failed to build up the conditioned response. After twenty-eight series containing not less than 2800 trials, there was not a single withdrawal from the contact when the light-flash alone was presented. We then continued to experiment with the subject in normal condition when he was fully aware of both the light and the shock. Contrary to what we obtained working with sound and shock, we now succeeded in establishing the conditioned response. Beginning with the fifth series and all through 20 following, there was withdrawal every time the nonadequate stimulus alone had been given. After 25 series of experiments in normal condition we again brought our subject in a post-hypnotic condition where he was blind for the lamp. Immediately every withdrawal response to light without shock completely disappeared, this in spite of the fact that we had established a very strong habit of withdrawal on light without shock during the experiments in normal condition.

Here we had to interrupt the experiments with subject V. as the term had come to its end.

The experiments with the second subject did in general run in the same way as the ones with Mr. V. We could, however, only use sound and shock as stimuli for the reason that we were unable to make this subject blind for the lamp.

The experiments with the third subject Mr. N. began in the same way as before. We tried to establish a conditioned response in post-hypnotic condition when the subject was deaf for the non-adequate sound stimulus. As before we failed to build up the conditioned response. We then continued with experiments in normal condition, and now we could establish the conditioned response easily. In this subject, there was no outlasting of an inhibitory tendency from the first phase of experimentation. We made a number of series alternatingly in post-hypnotic condition and in normal condition. Whenever there was deafness for sound all withdrawal responses on sound without shock disappeared. Whenever the subject perceived the sound there was withdrawal on sound without shock.

After this we tried an entirely new experiment. We wanted to see if we could condition a response by a positive hallucination, that is by a fact of awareness that is not due to real perceptual stimulation but due to suggestion only. The following method of investigation was used. We hypnotized the subject and gave him the suggestion that when he woke up he would see a small electrical lamp light up every time he got an electrical shock. This lamp was placed somewhat temporal in front of the subject. Our plan was this: if we could make the suggestion effective, every now and then, to really light up the lamp without giving the shock, and see if the subject would withdraw his finger on real light stimulation alone without shock. During the conditioning, of course, we would not light the lamp, simultaneously with the shock, or in other words, the conditioning stimulus would be merely a positive hallucination of the flashing up of the lamp. We succeeded in making the suggestion effective, that is the subject claimed in a post-hypnotic condition that every time he felt the shock he saw the lamp flash. The series of experiments we now carried him through were of the following kind. Each series contained as before 100 trials. Of these trials 80 consisted of stimulation with shock alone accompanied by hallucinatory light, 20 of stimulation with actual light alone without shock. On all the stimulations with light alone, except twice, in the first series of 100 trials, the subject withdrew his finger from the electrical contact. We repeated the experiment on several consecutive days, and found there was always withdrawal when the light was really lit up *without* the shock being given. We had consequently established a conditioned response to actual light on the basis of a positive hallucination.

It is interesting to compare these experiments with the previous ones. In our first experiments the negative hallucination or the anesthesia seems to associate itself, by some principle, with a negative behavior tendency (inhibition of withdrawal to the non-perceived stimulus) while in the last experiments the positive hallucination associates itself with a positive behavior tendency (withdrawal when the light is really lit up). It may not from one point of view be correct to speak of the former behavior tendency as negative, as the inhibition of an impulse to withdraw undoubtedly is also a positive tendency, and probably rather strong as such. When we speak of it as negative we only mean that it is so from the point of view of its overt expression (absence of withdrawal from the contact). The finding of the positive antipode to the phenomena occurring in anesthesia seems to us to give further validity to the principle that the suggestion of a rather simple sensory anomaly comes to involve, by a process within the organization of the human mind itself, rather far-reaching action tendencies.

The last phase of experimentation with subject N. was as follows: In a fully normal condition we proceeded to build up a conditioned response of withdrawal from the electrical contact to light stimulation. This conditioned response, was fully established after the first series of trials of 100. We carried, however, the subject through 300 more trials. Then we brought him into a post-hypnotic condition in which he was blind for the lamp. In 100 trials, now made, there was not a single withdrawal on light without shock, that is the tendency of the conditioned response was entirely inhibited. We then hypnotized the subject again, obtained rapport with an integrate of personality who admitted the inhibition, and gave to this integrate the contra-suggestion not to inhibit any impulses to withdraw during the experiment. Then we woke him up again in the post-hypnotic condition when he was blind to the lamp and continued the experiment. We had expected that he would now claim that he did not see the light and still would withdraw his finger from the contact every time the lamp was lighted, even when no shock accompanied this. What happened was however this: The subject kept raising his finger and put it back on the contact continually without any relation whatsoever either to shock or lighting of the lamp. This behavior continued as a kind of tic long after we had stopped both giving the shock and flashing the light. We thus failed in our endeavor to split up the mechanism set up by the suggestion of anesthesia.

The response we had been studying, that is the withdrawal of the finger from the contact, is not a true reflex. It is, without very much doubt, a semi-voluntary response. Would the same modifications by positive and negative hallucinations that we have studied in connection with this response, have occurred if we had had to do with true reflexes? From all general knowledge we have of clinical observations of hysterical anesthesias we would judge not. As a small side-issue investigation bearing on this point we made a study in two of the subjects of the behavior of the pupillary reflex and the eye-turning response—the latter is semivoluntary—on the one hand during stimulation by light for which the subject was blinded by suggestion and on the other hand during the condition when the stimulation was only a positive hallucination. The result was as follows: Whenever there was blindness for the lamp there was no eye-turning response when it was lighted, while the pupillary reflex always took place. Whenever the lighting up of the lamp was a positive hallucination coinciding with some other stimulation in another sensory field, the eye-turning response towards the lamp took place but there was not the faintest indication of any pupillary reflex. Observations concerning these matters were made independently by three observers and they all agreed to the facts mentioned.

Mrs. Lundholm and I have continued the experiments during the fall of 1927, and during this year, on a new subject, Mr. S., a student of psychology at Harvard, who has kindly volunteered as subject. The difference in our experimentation with Mr. S. has been that we now started to condition strongly a withdrawal of the finger from the electrical contact on the flashing up of the lamp during normal state. The conditioned response was easily established but to strengthen it we carried the subject through numerous series of trials on several consecutive days. Then we produced blindness in post-hypnotic condition for the lamp, and immediately every withdrawal of the finger on light without shock completely disappeared. In conditioning the response we had this time kept fairly equal intervals between the stimulations, and we had noticed a tendency in the subject to make a beginning withdrawal on the expectation of stimulation; in other words a rhythmical factor had been involved in the conditioning. This tendency to withdraw on expectation disappeared entirely in the post-hypnotic condition, where there was blindness for the lamp, which seemed to us again to indicate perhaps even stronger than previous evidence that with the suggestion of the anesthesia there

is really set up a co-conscious inhibitory tendency, relative to the withdrawal. In inducing the blindness for the lamp we had an experience that may be of some theoretical significance. The subject was brought to sleep and the suggestion given to him, read as follows: "You remember the experiments we have just been doing?" (This question referred to the series of trials in normal condition, given immediately before the hypnotizing. The subject answered in affirmative.) "Do you remember the little lamp you saw flash up during the experiment?" (Yes.) "When I wake you up in a little while and we continue the experiment, the lamp will not be there. You will not see it any longer." After we had awakened the subject we lighted up the lamp repeatedly to assure ourselves that he did not see it. In doing so we noticed that there was no eye-turning response towards the lamp when it flashed up, yet the subject's behavior indicated definitely that some kind of experience coincided with the lighting up of the lamp. On our question what was the matter, the subject told us that *something had been happening, he could not tell us what and where, except that it seemed to him to be in his eyes. Something like a flash.* We hypnotized the subject again and repeated the previous suggestion, adding a few words to the effect that when the subject now woke up this flashing in his eyes would also have disappeared entirely. When we had awakened him and again flashed the lamp a number of times he told us that even now something had been happening, but this time it was absolutely impossible for him in spite of strong urging to find any words to describe what had happened. The subject seemed to have had an experience carrying a minimum of meaning. The question raised by the observations is this: Is the effect of suggestion of anesthesia essentially an elimination of factors, without which an experience loses its proper meaning? It seems to us as if, by our first suggestion, certain factors of meaning had been blocked off from the experience of the subject, and as if, by the second suggestion, still further factors constituing the meaning had been blocked off. If this question can be reasonably answered in the affirmative a second one comes up. Are the hysterical anesthesias of the same nature? We would be interested to hear men with extensive experience both of hypnosis and hysteria, like Professor W. McDougall and Dr. Morton Prince comment upon these problems. When we carried on the experiments in post-hypnotic condition the subject was anesthetic for the nonadequate stimulation only to the extent described. In spite of this the withdrawal on

light without shock entirely disappeared and there was not the vaguest tendency during the post-hypnotic state of a reconditioning of the withdrawal by this experience with a minimum of meaning. On a number of consecutive days we continued to experiment with the subject alternatingly in normal and in post-hypnotic condition as above. Whenever the subject was aware of the flashing up of the lamp the conditioned response to light without shock always occurred, whenever his awareness of the flashing up of the lamp was an experience with a minimum of meaning, every single withdrawal on light without shock disappeared.

During January of this year, we have tried various modifications of our experiment. More interesting than the rest seems to us to be the result of the last session. We induced a periodically re-occurring anesthesia for the shock in post-hypnotic condition. The suggestion given to the subject during hypnotic sleep was as follows: "You remember the experiment we have just been doing?" (This refers again to a series of experiments in normal condition immediately preceding the hypnotizing of the subject. The subject answered our question in affirmative.) "When you wake up again, in a little while, we will continue to experiment. You will then see the light flash up again at certain intervals, as before, but you will get a shock only every fourth time the light flashes up. The very first time you see the light flash you will get a shock, then there will be three times of flashing of the light without shock, then the fourth time the light flashes you will again get the shock, then there will be three times of flashing without shock, then again a flashing of the lamp with shock and so on. Did you fully understand what I told you?" (Yes) "Repeat it." (The subject repeated the suggestion correctly.)

Then we woke him up and the results we now obtained were as follows: (Plus indicates withdrawal, minus absence of withdrawal. All trials were stimulations with both light and shock): +———+———+———+———+———+———, and so on indefinitely, with perfect regularity. In devising this experiment we had conceived of the possibility of obtaining just the result we got, but we had also taken into consideration another possibility, namely, that the effect of the strong conditioning of withdrawal from the contact by light without shock, that had been established during the thousands of trials in normal condition, might be felt in the post-hypnotic condition, and, there might express itself in a tendency to withdraw also on the three inter-

mittent stimulations when the shock was not sensed by the subject. The reason why this alternative was excluded as the experiment actually evolved itself became evident when we questioned the subject, while he was still in post-hypnotic condition, concerning his experiences. The essence of what he told us was this: He had never seen the light flash up except when he felt a shock and he voiced spontaneously his surprise that in this last experiment the intervals between the occurrence of shock plus light had been so very much longer than during all previous experiments. This final outcome of our suggestion seems to us interesting from two aspects: (1) In spite of the fact that the suggestion given to the subject,—a suggestion that he fully understood while asleep,—most definitely implies that he is going to see the light at times when he does not get the shock, the following thing takes place when he comes into the post-hypnotic condition: The anesthesia for three shocks out of four, which undoubtedly is there, as the subject does not withdraw on these three shocks, has made this part of our suggestion completely noneffective, resulting in the fact that the subject in the post-hypnotic condition becomes blind for any flashing of the light that does not coincide with the instance of a felt shock. We venture to believe that this association of anaesthesia for the shock with blindness for the light-flash is a result of the extensive experimentation in normal condition. (2) The regularity of the sequence of pluses and minuses indicates that the flashings of the light, that have not been consciously perceived in the post-hypnotic condition, yet must have been experienced by some system of the subject's personality. These flashes must have been preceived and more than that, they must have been counted by a consciousness that was co-conscious to the post-hypnotic personality-system.

We now decided to make another attempt to break up the tie between the anesthesia for shock and the associated blindness for the light. We hypnotized the subject again, and repeated our previous suggestion, emphasizing, however, very strongly that the subject really will see the light flash up three times without getting the shock. After we have had the subject repeat the suggestion, showing that he fully understood it we woke him up, and continued to experiment in the post-hypnotic condition, just as above. Every stimulation was thus a stimulation with both shock and light. The result we now obtained was as follows: Every fourth instance of stimulation, beginning with the first there was a clear withdrawal of the finger from the contact. The three inter-

mittent instances of stimulation when the subject saw the light but was not aware of any shock (this is his experience according to what he tells us himself after the series is through) he sometimes withdrew his finger definitely, sometimes there was a half withdrawal, or a jerk of the finger, sometimes there was no withdrawal at all. During the whole period of this experiment the subject showed the most definite signs of discomfort. We feel inclined to interpret the subject's inconsistent reactions and also his discomfort as indicative of conflict, and we venture to suggest, tentatively, that this conflict is between a tendency to inhibit withdrawal, set up by the suggestion of anaesthesia for the shock and a tendency to withdraw on light without shock, built up to a high degree of strength by our extensive experimentation in normal condition. In the experiment preceding the last one, this confiict was solved by a spontaneously occurring dissociation, opposed to our suggestion, the blindness for the light-flashes; in the last experiment, this dissociation was prohibited by our emphasis on a crucial point of the suggestion, and thus the subject came to suffer conflict. May not the first experiment perhaps be a laboratory parallel to certain types of hysterical disorder with dissociation, while the latter may be a laboratory parallel (why not?) to neurasthenia? We apologize for the boldness of such a suggestion, but we feel compelled to make it.

Again we have had to interrupt the work, as the examination-period approaches for our subject. We expect to continue the experiments with new subjects as soon as such are available, and we have devised various further modifications of the conditioned-response method, that we expect will then give further data. To summarize the result already obtained the following may be said: Functional anesthesia, as induced by hypnosis, seems to be, not merely a passive dropping out of a sensation-pattern from consciousness, but a condition involving a considerable amount of co-conscious conation, indicative of considerable co-conscious effort. This conation, as displayed in our experiments, appears to be, essentially, a co-conscious striving to act as if a sensory disorder is there. This striving to act as if, has been thwarted experimentally during ur laboratory work, and we have had an opportunity to observe, both successful overcoming of the thwarting tendencies, escape-means of avoiding them by spontaneous hysteria-like? dissociation, and conflict with neurasthenia-like? discomfort, due to incomplete success in overcoming the enemy tendency and failure to solve the conflict by dissociation.

2. *Suggestion towards a theory of functional anesthesia.*

Any student of functional anesthesias, confronted with the obscure inconsistencies and sometimes paradoxical expressions of these disorders has, undoubtedly, felt like a child in a deep forest out of which no route seems to lead. Seeking light the discouraged scholar turns naturally to Pierre Janet, the king among the now living observers of dissociation, and Janet offers him a theory the essence of which is, in short, the following: Functional anesthesias occur in persons of a specific make up, the hysterical, and is there determined by a lacking mental tension, or lacking capacity of keeping organized in one personal hierarchy all the functions of the normal mind. Due to this lowered mental tension certain functions like, for instance, the function of sensing from the right hand, may drop out of the personal consciousness. The scholar may be satisfied with this view of just a passive dropping out of a function, but, if he has been trained in the "hemicycle" of Hormic Psychology, he may intuitively feel that there must be a positive conation behind the anesthesia, and to determine this positive conation may become the "Golden Fleece" of his ambition, when he again sets out on the Argonautic Trail.

Above is related exactly the experiences that inspired us to the experimental study of functional anesthesias that has been reported.

Looking back on the experiments, at the stage to which they have now proceeded, we feel confident that by suggesting the various anesthesias we have caused more than a passive dropping out of a function from the personal consciousness,—our suggestion has definitely set up a positive tendency, that becomes co-conscious in the post-hypnotic condition, of acting as if there were the sensory disorder; this tendency expressing itself in a definite co-conscious inhibition of habitual reactions.

The discovery of this tendency did not, however, in itself lead us to the tentative speculation concerning the nature of functional anesthesias that is going to be submitted here. There came in addition another observation that is reported on page 346 in the discussion of the experiments. This observation seems to imply that with one of our subjects the effect of a suggestion of post-hypnotic blindness for a lamp, may essentially be that, by some or other principle, the suggestion deprives the experience of the lighting of the lamp of the factors that give this experience its full meaning even reduces it to an experience carrying a minimum of meaning.

As this occurred to us we naturally began to consider the nature of the meaning of perceptual thought.

Professor McDougall, in his discussion of meaning, brings out that the meaning of any object of thought, in its widest sense, is all we know of the object, including its relation to numerous other objects. But the meaning of an object, the moment we do think of it, is mostly very much more limited, and is determined by the striving at work when we think the object. It seems to me that Professor McDougall has brought conclusive evidence for the truth of this by the illustration he gives in his "Outline of Psychology". I quote from this work a few passages to bear on the matter:

"Now note, that when I perceive the orange, there is somehow operative in the act of perception not only my previous experience of oranges, constituting the act one of recognition, but also a vast amount of other experience, experience of material objects of many kinds—experiences of seeing, of handling, of tasting, of hearing, and so forth. All this mass of past xperience contributes to the "meaning" of the object, or, more strictly, my "meaning" as I perceive the orange. . . .

"Of the total 'meaning,' the total knowledge which is operative in the mind as I perceive the orange, one part or another is apt to be more prominent and to play a larger part than others in the guidance of my behavior, according to my purpose at the moment my glance falls on it. If I am thirsty and seeking to allay my thirst, I seize the orange or, at lease, perceive it as an object that will satisfy that desire; I perceive it as a succulent cool fruit; that part of its total 'meaning' is most prominent. If I am looking for a missile to throw at an intruding burglar, I probably pass it by. But, if I am looking for one to throw playfully at a friend, I may seize it for that purpose; then its weight, softness, its property of going 'squash' on impact, are the prominent part of the 'meaning' I find in the visual impression."

So far, Professor McDougall. Now another consideration: Is it not so that any perceptual thinking of an object is determined by striving? Is it not so that as far as an impression awakens a striving, or as far as it sustains a striving, it activates perceptual thinking? Only such an impression makes us attend to it and makes us fully cognize the object. Other impressions, non-related to any striving, do not activate perceptual thinking, they leave us unaware or anyhow very vaguely aware of their occurrence; they furnish only a very indistinct background pattern of awareness,

in which each part carries a minimum of meaning. I think I would be able to quote Professor McDougall also here for support of this view.

Let us now return to the experiments.

I emphasize again that, by our suggestion of an anesthesia, we apparently set up a tendency to act as if there were a sensory disorder. This tendency, we assume, with Professor McDougall, generates energy from the instinct of submission. Let us for the sake of a theory consider this tendency to be the primary result of the suggestion. This tendency becomes co-conscious in the post-hypnotic condition, but it serves continually to inhibit in the post-hypnotic personality any striving in relation to impression by the non-adequate stimulus. Now, the post-hypnotic personality is non-aware of this inhibitory tendency, yet he is, by it, incapacitated of any striving relative to a certain impression. How would this situation influence his awareness of latter impression? Would it not, in accordance with our previous general discussion of striving and meaning, strip this impression of all the factors constituting its meaning and leave it an experience carrying a minimum of meaning? If the co-conscious inhibitory tendency is fully successful, would not then the impression be deprived of meaning to an extent to make its value in the sphere of personal consciousness equal null? Might not this be exactly the nature of the anesthesia our subject suffers?

The theory we thus suggest concerning the nature of functional anesthesia, as induced by hypnosis, may in short, be restated as follows: The suggestion of an anesthesia primarily builds up a tendency to act as if this defect were there. This tendency which becomes co-conscious to the post-hypnotic personality, inhibits in him any striving in relation to impressions received through a certain sensory area. This deprives such impressions of meaning to an extent to make the subject suffer an apparent anesthesia for them.

It seems to us as if this theory may be inversely applied to the explanation of positive hallucinations as induced by hypnosis. When we suggested to our subject, that any time he got a shock he would see a lamp light up, we probably set up in him a tendency to strive as if this lamp really flashed (one manifestation of this tendency was later the eye-turning response that mostly took place whenever the shock was applied). In the post-hypnotic condition this tendency became co-conscious, but it continued to be activated every time the shock was given. This striving determined cogni-

tion relative to the lamp, and added to the impression of the non-lighted lamp such factors as to induce in the subject the meaning of "lamp lighting up"—a beautiful illustration, if our interpretation is true, of the principle that conation may determine cognition.

There is one feature of the experiments that apparently seems to go against the theory now offered. In two of our subjects we found an outlasting into a fully normal condition of the inhibitory impulse relative to withdrawal on sound without shock, that had been previously strongly established during extensive experimentation in a post-hypnotic condition when the subject suffered artificially induced functional deafness. If this inhibitory impulse continued to act co-consciously and prevent the building up of the conditioned response in the normal condition, why did it not produce deafness in the normal condition or in other words, why did it not throw the subject back into the condition of the post-hypnotic period. We feel unable to answer satisfactorily this question, and humbly admit that it is a blow to the theory. It may however be that the existence of this co-conscious inhibitory tendency effected some modification of meaning of the subject's sound impression in the normal condition; not enough to extinguish it entirely as sound, but enough perhaps to prevent the subject from clearly cognizing the coincidence of sound and shock. This is, however, only a guess on loose grounds, and, we admit it, a poor contra-argument in favor of the theory. If we had known what the further experimentation would lead us to believe, we would have investigated the nature of our subjects cognition relative to his sound impression during the period of experimentation in "normal" condition. Here is a problem of further research if we would again, in a new subject, meet this outlasting of the inhibitory impulse during such condition. We do not feel that the contra-indication relative to the truth of our theory, that is discussed in this paragraph, necessarily compels us to give up the theory but it compels us to emphasize again its tentative nature.

And now a few words about the spontaneously occurring anesthesias in hysteria, in the light of our theory.

May it be possible that also the latter anesthesias are determined by analogous principle as the one we have suggested relative to anesthesias, artificially induced by hypnosis? We are inclined to think: Yes. The main difference would only be this, that in hysterical anesthesia, the co-conscious inhibitory tendency which prevents the conscious personality to strive in relation to

certain impressions, does not generate, necessarily, energy from the submissive instinct, but from some or other different wish, as for an instance, a wish to escape, or a wish to obtain sympathy and through it any other desired goal.

If our theory concerning functional anesthesia is true, it opens a relation between the fairly simple phenomena of functional insensibilities and very complex psychical obsession. To prepare for the understanding of this, permit me to tell a story, or rather to relate a memory.

I used to have a friend who was an autodidact. Independent of any guidance by high school or college, he had acquired a considerable amount of information concerning matters of philosophy and the psychology of religion. He and I frequently met in his studio, and, while the crystalline twilight of the Nordic summer eve slowly fell, we used to discuss various questions of psychology. As I retrospect now on our sessions, I can recall how, on certain occasions when he was in a particular state of mind I used to be annoyed by the obstinacy with which he seemed to oppose any suggestion attempted, whether it came from me or from somebody else. It seemed to me as if any positive statement of mine, concerning whatsoever, immediately set up in him an ambi-tendency to think differently. I sometimes pointed out to him this oppositional obstinacy of his before we separated after a hot discussion. On such occasions he seemed to be only vaguely aware of it himself, and only when I reviewed the course of our conversation did he grant I was right, and seemed to be a little ashamed. Another thing was also striking: Whenever he was in the ambi-tendencial state of mind he lost considerably his capacity to grasp an argument, this to a point that he sometimes thought that he was defeating me while he in fact expressed a view that I had just before approved of. When I interrupted to tell him that, what he had been arguing for, during the last five minutes was exactly what I thought myself and had expressed previously, he showed signs of embarrassment or surprise.

May it be that the principle underlying this singular tendency of my friend was the following: He was a man who, without having been favored by sympathetic guidance, since boyhood up had sublimated a strong inferiority-sentiment in striving for intellectual acquisitions. This is so far only sound sublimation, but, as time goes on, the inferiority-sentiment begins to generate energy into the less sound compensatory channels of superficial assertion and pride. And now the inferiority-sentiment, which

becomes less and less conscious, has set up a tendency that is detrimental to continued intellectual development, a tendency that is essentially inhibitory to further acceptance from others, even to curiosity concerning other people's views. This tendency becomes activated by any positive intellectual impression from others, drains into other channels the impulse that should go into curiosity and attention relative to the impression, and thus impairs the capacity to understand the full meaning of an argument.

If this rapid sketch of the psychogenesis of my friend's obsessional ambi-tendency on the intellectual plane is correct, is not then his case in a way analogous to functional anesthesia? Have we not here, in a less extreme degree, a modification, an enpoverment, of the meaning of objects of thought by an assumably co-conscious, or, anyhow, far from conscious tendency that is inhibitory of proper striving relative to certain social impressions.

My friend's case can, without doubt, be stated as a case of prejudice, and, as the principle underlying functional anesthesia, in a wide sense, can be applied to it, this principle can, I think, in the same sense be applied to any case of prejudice. (Note the expression: Blind prejudice.) This extends again the domain of the principle through a range of increasingly maladjusted prejudices unto the delusions of certain insane, such as the paranoid and the schizophrene. In these extreme instances, just as in any case of prejudice, there are probably co-conscious tendencies, generating energy from an all-powerful egotistic wish, which modify normal conscious striving, thus changing the meaning of things. In the schizophrene the impressions of the objective world in toto becomes, by such principle, deprived of the factors of meaning, constituting its reality-criterion, to an extent that the line of demarcation between reality and fantasy extensively dissolves. There occurs, as a result of the activity of such co-conscious tendencies (why not?) an anesthesia, a blindness for the reality criterion, or in other words, the function of discriminating between reality and fantasy becomes dissociated.

Waverly, February 9, 1928.

SEX DIFFERENCES IN CREDULITY

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POPULAR thinking assumes conspicuous differences between the sexes in most traits. The nature and the extent of sex differences in abstract intelligence have received the attention of numerous psychologists. Terman, Hollingworth, Whipple, and others have presented and discussed sex differences in this regard.(1) Summaries and discussions of miscellaneous studies of sex differences have been published recently by Allen, by Boynton, and by Goodenough.(2)

Thorndike pointed out many years ago that the most striking fact about the sexes in most traits is the similarity as revealed in the amount of overlapping of distribution surfaces.(3) Miss Goodenough in a recent summary of representative studies in sex differences asserts: "The most outstanding impression which one gains from a comparison of these studies is the inconsistency of the various findings." (2 c, p. 441) The inconsistency of the earlier findings was perhaps due in part to crudity of techniques. The more recent investigations which employ more refined technique reveal consistent sex differences in certain traits. Goodenough summarizes the findings to date:

"A slight superiority of females over males in general linguistic ability has been reported by practically all investigators in this field." (2 c, p. 444)

"Studies made in the upper elementary grades and in high schools and colleges show a consistent difference in favor of the boys on practically all tests involving the use of number." (2 c, p. 449)

"Data from a large number of sources show a consistent superiority of females over males in memory tests." (2 c, p. 450)

"The weight of evidence . . . seems to point to the conclusion that boys have acquired a truly wider range of general information than have girls by the time they arrive at the beginning of the grammer school period, and there is some reason to believe that some degree of sex differentiation in this respect may be observed even during the pre-school period." (2 c, p. 453)

"That boys and men on the average . . . greatly exceed women and girls in the ability to manipulate mechanical contrivances is not only a matter of common observation but has been brought out very clearly in an extensive investigation on me-

chanical abilities which has been carried out at the University of Minnesota, the results of which will be published in the near future." (2 c, p. 453f)

A common practice in dealing with sex differences in intelligence is to consider differences in total or composite performance upon a number of tasks calling for widely varying mental reactions. The expression of the total performance is usually in terms of M.A. or I.Q., blanket terms including many varied and totally unlike cognitive responses. Terman finds that girls do slightly better in intelligence tests than do boys of the same C.A. until the chronological age of thirteen is reached. Miss Goodenough believes that the slight superiority of girls is due to the fact that " . . . more than half of the total number of items in the section of the Binet under consideration are chiefly upon types of performance in which females have been found to exceed males at all ages for which information is available, while the types of performance in which males have usually been found to exceed females are practically unrepresented.(2 c, p. 458) Because of the conspicuous differences in the types of test items employed in the intelligence test, "*the question of mental precocity as related to sex must be answered in terms of specific functions or traits, rather than in terms of unanalyzed general tendencies.*" (2 c, p. 459.)

Conklin suggests that most of the experimental methods are not well designed for the study of those traits in which sex differences are most likely to be found, namely, the affective processes.(4) Thorndike, like Conklin, also believes that the most important effector of differences between the sexes in many types of behavior is to be found in differences in emotional equipment and response. He estimates that only 30 per cent of men equal or exceed the median of women in emotionality.(5) E. K. Strong, Jr., found that women were very much more easily aroused emotionally than were men.(6) Pressey and Pressey contend that greater emotionality is displayed by women than by men in responding to a "cross-out" test,(7) and the writers have shown that girls are aesthetically more demonstrative and appreciative than boys.(8)

Marston found that more girls than boys may be characterized as "introverts." (9) Laird reports that the tendency toward introversion is so much more marked in women that different percentile scales are necessary for the two sexes. For example, a man whose raw score is 15 receives a percentile rank of 90, and a woman with the same score receives a percentile rank of 50; similarly a per-

centile rank of 2 for women corresponds to one of 30 for men, and a rank of 99 for men corresponds to one of 87 for women.(10) By varying the method of measuring introversion and extroversion Heidbreder found no general difference between men and women in introversion and extroversion.(11) However, in Heidbreder's study certain sex differences in temperament appeared, aside from the matter of introversion and extroversion.(11, p. 54) Heidbreder states that women are more likely than men to exhibit the following types of behavior: (11, p. 57f)

1. Shrinks when facing a crisis.
2. Works by fits and starts.
3. Has ups and downs in mood without apparent cause.
4. Feels hurt readily; apparently sensitive about remarks or actions which may have reference to himself.
5. Hesitates in making decisions on ordinary questions in the course of the day.
6. Dislikes and avoids any process of selling or persuading anyone to adopt a certain point of view (except in the religious field).
7. Worries over possible misfortunes.
8. Is absentminded.
9. Admires perfection of form in literature.
10. Keeps a diary.
11. Prefers to read a thing rather than experience it.

Heidbreder reports that men are more likely than women to be characterized as follows: (11, p. 58)

1. Is outspoken; says what he considers the truth regardless of how others may take it.
2. Prefers to work a thing out on his own hook; hesitates to accept or give aid.
3. Is thrifty and careful about making loans.
4. Keeps in the background on social occasions; avoids leadership at social affairs and entertainments.
5. Prefers participation in competitive intellectual amusements to athletic games.
6. Is meticulous; is extremely conservative about his dress and painstaking about his personal property.
7. Introspects; turns his attention inward toward himself.
8. Is slow in movement.

Most of the sex differences which were found by Heidbreder suggest a masculine-feminine antithesis of aggressiveness and dependence.

Differences in the affective processes might be expected logically to be reflected in differences in certain types of behavior

usually catalogued as belief. Indeed, Lund attributes the origin and dominance of certain beliefs to differences in emotional equipment and response.(12) That sex differences in certain beliefs and superstitious credulity may be traceable to differences in the affective processes seems logical. If the intimacy of association between emotional reaction and belief be recognized, one would expect discernible differences to exist between men and women in the number of and variety of beliefs. Indeed, several studies have shown that women and girls *are* more credulous in general than are men and boys. Nixon studied the reactions of 140 women and 219 men to a list of 30 popular superstitions.(13) The subjects were students in elementary psychology. All of the statements in Nixon's list were false. Only 41.1 per cent of them were marked true by the young women and only 35.1 per cent by the young men.

Garrett and Fisher lengthened Nixon's list to 40 items and studied 140 senior girls in the Washington Irving High School, and 115 senior boys in the De Witt Clinton High School of New York City.(14) The average number of statements marked true was 17.6 for the boys and 19.9 for the girls, a difference of 2.3. This difference is small but Garrett and Fisher believe that the difference is fairly reliable and probably representative of a true sex difference.

Conklin studied the superstitions of 267 boys and 290 girls.(15) The subjects were American college students of ages 16 to 25 inclusive. Conklin realized that many individuals might be reluctant to admit *belief* in commonly practiced superstitions.[1] However, he felt that a superstition sufficiently strong to influence human conduct is worthy of consideration. He therefore instructed his subjects to record *all* superstitions in which they believed or which they felt had influenced their conduct. He asked his subjects also to report the superstitions in which they had believed previously. Following are some of the more pertinent of Conklin's findings:

1. Superstitious belief or practice was indicated dominant at the time of the investigation by 40 per cent of the boys and 66 per cent of the girls.

2. Ninety per cent of the boys and 73 per cent of the girls had immediately recallable experience of earlier superstitious belief or practice.

[1] For example, knocking on wood to avoid the consequences of boastfulness might be practiced merely in a spirit of fun, or because it seems to be the conventional thing to do, or because it represents an infantile habit not replaced entirely by a new one.

3. The boys appeared to outgrow superstition at earlier ages than the girls.

4. Slightly more superstitions per individual were mentioned by girls than by boys.

5. One-half of the subjects assigned their former superstitions to the years 12 to 16 inclusive.

6. Fifteen per cent of the girls and only 5 per cent of the boys mentioned "fortune telling (cards, tea-cup, palm)" among the superstitions believed in or practiced *at the time of the investigation.*

7. Fifteen per cent of the girls and only 8 per cent of the boys mentioned fortune telling among the superstitions *formerly* believed in or practiced.

Collectively, the various investigations cited thus far show that girls in general are somewhat more credulous than boys. The credulity reflects itself in several ways. Heron found college women to be consistently more suggestible than college men in reporting the presence of warmth when heat was not present.(16) The subjects were instructed to grasp a coil of wire between the thumb and forefinger. They were told that the coil would be gradually heated by an electric current and that the experiment was for the purpose of determining the threshold of warmth perception. More than 90 per cent of the subjects experienced illusory warmth. However, the women were consistently more suggestible in this regard than the men.

The credulity of girls and women reveals itself frequently in another easily accessible form of behavior, namely, participation and probable belief in the results of "fortune telling". It is an open question whether the practice of having one's fortune told is attended by acceptance of the postulates or the results. The results of the investigations discussed thus far are based upon limited numbers of somewhat selected students.[2]

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[2] Subsequent to publication of the above findings Conklin wrote the following under the heading "A Further Word on Superstition."

"Since publishing my study of the superstitions of college students (this Journal, XXX, 1919, 83), I have discovered a somewhat similar study made over thirty years before and published under the rather misleading title of 'First Report of the Committee on Experimental Psychology' (*Proc. of American Society for Psychical Research*, I, No. 3, 1887). . . .

"Isolation of the committee's returns from adolescents, for comparison with my own returns, indicates that the committee found about 11 per cent of young men with a superstitious tendency and about 17 per cent of young women. My results for the same age-group were 40 per cent and 66 per cent, approximately the same ratio. That two studies made thirty years apart, in localities three thousand miles apart (New England and Oregon), and by somewhat different methods, should indicate the same degree of difference between the sexes adds much to the reliability of the conclusion." Conklin, E. S., "A Further Word on Superstition," *Amer. Jour. of Psy.*, 1921, pp. 158 f.

The writers have assembled data upon large numbers of unselected children which corroborate the findings of previous investigators and may be indicative of a *real and general* sex difference in one type of belief. The following paper presents: (1) A study of sex differences in "Telling fortunes or having fortunes told", and (2) a brief discussion of the results.

METHOD

Over 6,000 children were asked to check from a comprehensive and catholic list of 200 play activities *only those* activities in which they had voluntarily engaged during the preceding week. The directions were specific, the teachers were instructed in giving the tests, and the data were obtained from unselected school children in order that the results of the investigation might be reliable. Data were obtained from children in the following Kansas towns: Kansas City, Lawrence, Bonner Springs, and Moran. The older individuals were for the most part students of the University of Kansas.

In order that seasonal differences might be taken into account the list was checked on each of three different dates, Nov. 7, 1923, Feb. 20, 1924, and Apr. 30, 1924. The same procedure in administering the test was used in each testing. The total number of individuals from whom data were obtained is indicated in Table I.

TABLE I

NUMBER OF INDIVIDUALS INCLUDED IN THREE INVESTIGATIONS OF PLAY BEHAVIOR

	Boys			Girls		
Ages	Nov. 1923	Feb. 1924	Apr. 1924	Nov. 1923	Feb. 1924	Apr. 1924
8½	98	90	80	100	97	99
9½	169	161	144	174	139	144
10½	182	169	160	215	199	176
11½	187	167	184	235	222	220
12½	249	201	176	326	289	266
13½	280	231	259	269	235	263
14½	274	252	238	301	282	278
15½	130	247	247	261	244	256
16½	210	181	193	251	223	235
17½	145	130	146	182	208	193
18½	115	170	130	120	174	267
19½	95	119	102	101	115	93
20½	50	73	59	73	124	76
21½	53	57	43	41	85	66
22 or above	79	105	68	44	114	88

One of the items in the list was "Telling fortunes or having fortunes told". The pupils were directed to mark this activity if

they had engaged in it "just because they wanted to" during the preceding week.

RESULTS

Data were assembled separately according to sex. Tables were then prepared showing the percentages of children in each age group participating in "Telling fortunes or having fortunes told" during the course of one week preceding a given investigation.

Table II presents the data assembled by sex and by season.

TABLE II

PERCENTAGES OF BOYS AND OF GIRLS WHO PARTICIPATED IN "TELLING FORTUNES OR HAVING FORTUNES TOLD" DURING THE COURSE OF ONE WEEK PRECEDING A GIVEN INVESTIGATION

	Boys				Girls			
Ages	Nov. 1923	Feb. 1924	Apr. 1924	Ave.	Nov. 1923	Feb. 1924	Apr. 1924	Ave.
8½	11	6	4	7	15	5	5	8
9½	11	6	6	8	18	9	9	12
10½	11	8	3	7	13	11	5	10
11½	12	5	3	7	16	7	8	10
12½	2	6	2	3	16	13	11	14
13½	7	3	2	4	20	14	13	15
14½	7	2	1	4	20	11	9	14
15½	6	5	0	5	16	10	6	11
16½	7	3	0	3	16	8	6	10
17½	4	2	0	2	13	8	10	10
18½	2	1	0	1	16	10	4	9
19½	3	1	0	1	14	12	6	11
20½	0	0	0	0	11	12	5	10
21½	4	2	0	2	10	11	11	10
22 or above	3	1	0	1	18	11	7	11

Table II shows that, although the girls participated in fortune telling activities slightly more frequently during the fall season, the findings are fairly consistent from season to season. It will be noted further from Table II that sex differences in participation in the item under discussion were displayed consistently in all of the investigations. The sex difference is best revealed in Figure 1 which presents the composite findings for the three studies. Study of Table II and Figure 1 discloses:

1. That the girls participated in fortune telling activities more generally than the boys at every age level.
2. That sex difference is relatively slight from ages eight and one-half to eleven and one-half inclusive and most pronounced from ages twelve and one-half to fourteen and one-half.
3. That the girls participated most frequently in fortune telling

activities at ages twelve and one-half to fourteen and one-half inclusive.

4. That with increase in maturity there is a slight decrease in participation in fortune telling activities among the boys but there is not a corresponding decrease for the girls.

DISCUSSION

The data presented in Table II and Figure 1 corroborate the investigations of Nixon, Garrett *et al.* They show clearly that girls and women engage more often in fortune telling activities than do boys and men. They suggest that in general women are more

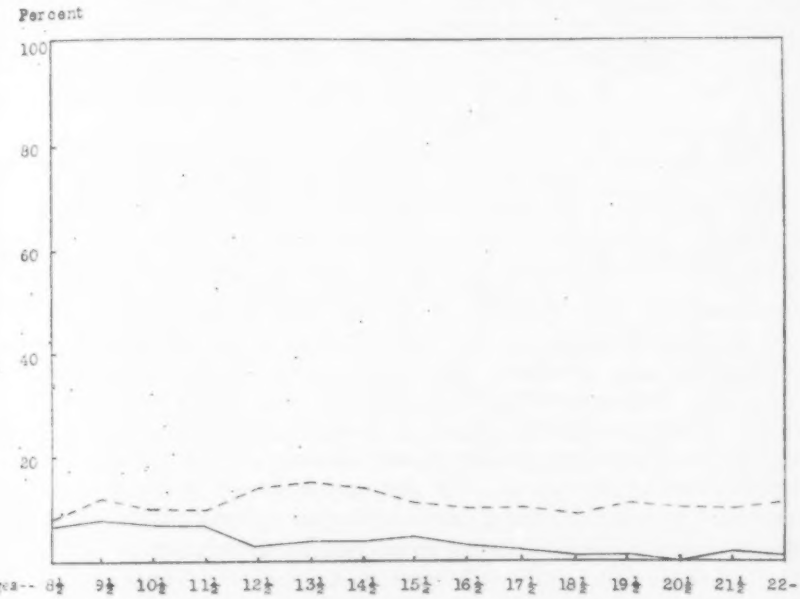

FIGURE 1

Percentage of boys and of girls who participated during the course of one week in "Telling fortunes or having fortunes told." Average of results obtained from three separate investigations.

Boys ———— Girls — — — — — —

credulous than men. It is of interest that the most marked sex difference is found at ages eighteen and one-half to twenty-two. One would expect maturity to be associated with a decrease in participation in ungrounded magic or superstition. A decrease in participation in fortune telling is shown in the data for the boys. Yet, girls persist in taking part in this activity as they mature.

It is true that the participation of both sexes is greatest at ages twelve and one-half to fourteen and one-half but the sex difference is almost as great at ages eighteen and one-half to twenty-two as at twelve and one-half to fourteen and one-half. The marked difference at the highest ages is due to the fact that increase in C.A. does not reduce markedly the frequency of participation in fortune telling activities for the girls. However, the decrease is marked for the boys. The result is that the greatest sex difference is found at ages eighteen and one-half to twenty-two. These data are particularly interesting when one considers that the more mature individuals were college students. One logically would have anticipated that higher education would effect a reduction in participation in superstitious activities. Therefore, if higher education does effect a reduction in this regard, a corresponding curve for non-college girls would reveal more frequent participation than that shown in Figure 1.

The significance of the above findings becomes more apparent when one recalls that Figure 1 discloses participation in fortune telling activities for one week only. The frequency with which this activity is engaged in would probably be higher than that revealed in Figure 1 if activity participation over a period of several weeks were studied. It is possible, of course, that some of the individuals took part in this activity without seriously accepting the premises upon which the practices are based. The spirit of these persons may have been comparable to that of one engaging in a play or game. This possibility, although it alters somewhat the interpretation to be placed upon the above findings, does not destroy the significance of the data. To the psychologist make-believe play activities are significant phases of behavior and adjustment. The writers have discussed these activities in a previous article.(17)

Although the older girls who took part in fortune telling may not have believed in the validity of the prophecies, it is probable that girls really are more credulous of the results of fortune telling than are boys. The relative frequency of participation of the sexes in this activity would suggest this point of view. Too, in class discussions and during personal conferences with students the writers have been impressed by the fact that it is usually a girl who cites instances in which (according to the student's professed belief) a fortune teller has made an accurate prognostication. Boys rarely offer such testimony.

It is of interest to attempt to account for the sex difference in credulity, and to speculate regarding its significance. In a recent volume the thesis is presented that the effort to maintain com-

placency characterizes most human behavior.(18) Raup states that lack of complacency produces a drive to activity and that, with the achievement of complacency, the activity ceases. Drever adheres to an almost identical point of view although his terminology differs somewhat from that of Raup.

"A recent school of biology and biological psychology sets out from the conception of a stationary physiological state, which the organism strives to maintain, or regain if it is disturbed. The designation 'stationary physiological state' is perhaps unfortunate, for in life there is no stationary state. But the fact so designated is nevertheless an obvious one. There is a condition of equilibrium to which the organism constantly tends to return. This equilibrium is not a mechanical, static equilibrium, but a vital, dynamic one. Every threatened or actual disturbance of the equilibrium evokes forces and activities which tend to maintain or to restore it." (19)

Dressler has adopted a theory similar to that of Raup and Drever in attempting to account for superstitions. Dressler states that superstition is a hygienic necessity in that it satisfies a natural demand for plausible explanations. Dressler believes that the acceptance of a superstition effects a satisfying mental state essential to mental equilibrium and quiescence.

"Superstitions represent in part those conclusions which men have adopted in order to free the mind from the strain of incompleted thinking. Men are naturally driven to conclusions regarding the meaning and significance of those phenomena which appear in their minds. There can be no physiological or psychological equilibrium unless the mind comes to rest in a conclusion. It is physically and mentally very trying to hold in the mind a series of conditions and at the same time prevent them from shooting together into some sort of dénouement. The untrained and the instinctive mind reaches conclusions quickly, for this temporarily is the line of least resistance." (20)

If the hypothesis presented by Dressler be accepted, the data herein presented suggest that girls may be less able than boys to stand the strain of incompleted thinking. This hypothesis provides a partial explanation not only for the findings in reference to fortune telling but also for certain other commonly recognized sex differences in behavior, namely, woman's alleged "intuition", her relatively great religious faith, and the ease with which she is proselyted by leaders in "new thought", spiritualists, etc., etc.

The question naturally arises: Why should the strain of incom-

pleted thinking be so much more intolerable to women than to men? That fact *seems* apparent that fortune telling offers a possibility for compensatory gratification, for it permits a mental quiescence to be attained. Why women should need to turn with such great frequency to this activity is a question of importance.

The writers have suggested that women may be less able to stand the strain of incompleted thinking. This, however, is a partial explanation for the high frequency of participation in fortune telling activities on the part of women. The writers have shown previously that women are restricted in activity participation in general.(21) This restriction may operate to produce a state of uncertainty and restlessness. Certainly, the thwarting of certain desires occurs more frequently for women and girls than for men and boys because of the restrictions placed upon the activity of the female. One avenue of escape which brings quiescence and satisfaction is the acceptance of superstition. With such acceptance, the fears and uncertainties are to some extent eliminated.

There are, of course, other factors which produce the sex difference. Some may assert that the strain of incompleted thinking is intolerable to women because of affective processes that operate to produce great emotionality. They may insist that women turn to such activities as fortune-telling because of the satisfyingness vicariously achieved through this activity for strong emotional desires or drives.

Others, however, may be reluctant to accept this hypothesis. They may assert that woman's lack of physical prowess and resultant dependence cause her to seek aid in supernatural or unnatural ways. The sex difference under discussion may be in part a result of the lack of physical prowess and emotionality of the woman. These conditions restrict the activity range of woman. However, there is evidence that other forces operate also to restrict the activity range of the girl or woman. The writers have shown that girls are obliged to confine their activities to a relatively small geographical radius.(21) It is entirely likely that such restrictions which are constant and numerous produce in the girl or woman a felt need for compensatory reactions. Superstitions offer easily accessible modes of adjustment by which satisfyingness and complacency may be achieved. How much of the sex difference (displayed in Table II and Figure 1) is due to emotionality, how much to volitional differences, how much to the restrictions of

behavior, and how much to other factors, is of course impossible to state. It is also impossible at the present time to state with confidence whether the sex difference in credulity is determined primarily by native constitutional differences or by early training and environmental influences.

The problem of credulity is extremely complicated and obviously it can hardly be explained in few words, or by *a priori* formulae. In the present paper the writers have attempted: (1) To show graphically that there is a significant sex difference in regard to credulity as expressed in the attitues of the sexes toward fortune-telling activities, and (2) to suggest that this sex difference may be due in part to woman's relatively great emotionality or to her restrictions. These forces may cause her more frequently than man to fall back upon feeling for guidance.

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PROBLEMS OF PERSONALITY

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IT is the purpose of this paper to attempt a brief description of personality and to outline its chief characteristics and problems.

In the treatment of the problem of personality by the ancient Greeks, Roback (13) points out that there were two divergent lines of early development, the one literary and the other scientific. The first is represented by the work of Theophrastus who described twenty different character or personality types, with unusual clearness and insight. Both Plato in his Dialogues and Aristotle in his Ethics made some notable contributions to the problems of character delineation. Chaucer in the early English period did something similar in his description of the characters in the Canterbury Tales. In the latter part of the Middle Ages character description was one of the more common types of literary endeavor with numerous characterologists in almost every country of Europe. More recently we have witnessed an attempted revival of this type of description in a somewhat different form in such novels as Babbitt and Elmer Gantry.

The other type of development, scientific rather than literary, was first begun by the Greek philosopher Hippocrates, but this work has generally been credited to the physician Galen who lived a few centuries later. This method of approach remained essentially unmodified until within the last half century. In fact, Galen's characterization of the four temperaments as choleric, phlegmatic, sanguine, and melancholic is still at least mentioned in most texts in psychology.

The modern movement in the scientific analysis of personality has had two or three separate developments. The first really began with the work of Galton and came to real fruition in the work of Binet. In America the work was taken up by Cattell and Jastrow and has been carried forward by Thorndike, Terman, and others. These researches were largely in the field of one personality trait, namely intelligence. More recently a host of other personality traits have been studied and measures constructed for them.

Another scientific development came from the Freudian school of psychology. Jung's (8) study of types is the best known of

these, but the whole psychoanalytic school has had a large part to play in the study of personality. Kretschmer's (9) study of types is partly analytical and partly physical.

If we were to name a third contribution which as yet has not progressed far we would mention the work of the endocrinologists. Crile (5) and Cannon (4) have contributed some interesting studies in this field. Rich's (14) work is closely related to these. This approach is yet so new that we are unable to evaluate its possible bearing on our problem.

This brings us to a statement of our first problem which is—What is personality?

The word personality has had various usages and has been used interchangeably with temperament, character, and self. There are two essentially different senses in which the word personality itself has been used. One is in the sense of the whole being. Warren, for example, states that personality is " the total outcome of an individual's permanent mental organization, comprising all his permanent mental conditions and organized experiences at any period of life." Watson gives a similar definition. Other writers such as Bernard, Allport, and Dunlap stress the social elements of personality. In this paper personality will be used in the latter sense. That is, to denote the relation of an individual to the social group. With this usage personality is to be contrasted with:

(1) Temperament, which by most writers is used to refer to the more stable, emotional tendencies of the individual.

(2) Character, which is sometimes used as synonymous with personality and sometimes implies more of the moral phases of conduct.

(3) Self, which generally refers to the whole individual. This is the usage of James, who classifies self into the material, the social, and the spiritual self. Personality is the same or similar to James' social self. While there is no inherent virtue in any definition other than that of clarity and delineation from other terms, it seems best to use the term personality to mean the social aspect of one's self.

There are many ways in which an individual may be analyzed or studied just as there are many different dimensions to any solid. Like the measurements of an irregular solid some of these mental measurements are more significant than others in a description of the individual. One aspect of this classification is the physical form of the individual. Any individual may be classified as to height, strength, weight, etc. Another classification might be his

general mental make up, that is, his sensory equipment, instincts, higher thought processes, etc. Closely related to this latter problem is one phase of his mental make up and that is the way in which he is related to other individuals socially. This is our specific problem in personality classification.

Our second problem is that of the origin of personality. Is it native or acquired or rather what part is one and what part the other? This problem presents no more or no less difficulty than other problems of heredity and environment. There are those who claim that personality is predominantly inherited. Others claim that it is largely acquired. Unfortunately we have as yet devised no successful technique for settling this difficult problem in any field. Terman and most eugenicists represent one extreme view. Watson in his more recent statements represents the opposite viewpoint. It is worth while in this connection to point out that the vehemence of their argumentation is no sure measure of the validity of their contentions. This problem still remains unsolved. Fortunately other features of the problem may be investigated while this problem remains unsolved.

A third problem closely related to the second is that of the evolution of personality. Even the staunchest contender for the innate native character of personality would admit that these native tendencies are modified by training. Others of us believe that by far the larger elements in personality are the result of training. The genetic development of personality is one of the most interesting problems of dynamic psychology. But this is too large a problem for our consideration here. Suffice it to point out that Child and Herrick have given us a very excellent description of the physiological foundations for this development while McDougall (10), Young (15), and Bernard (2) have traced in detail the psychological and sociological phases of this problem.

A fourth problem is the analysis of personality. What are the elements, or as they are usually called, the traits of personality? Here again opinions differ widely. Heymans and Wiersma (7) list 104 different personality traits. Brandenburg (3) lists about 40. Allport (1) lists 5 with several subdivisions under each heading. Manifestly personality is very complex and it would seem that any analysis of it would necessarily be rather complex. Yet one important basis of any classification is utility and certainly any long list of personality traits becomes largely meaningless and useless due to overlapping of traits and the lack of clear-cut distinctions between them. Therefore it seems more important to

determine the large factors in personality which are clear cut and mutually exclusive and independently variable, admitting that any such classification may not be logically complete. But such a list has the advantage that at least it may be workable. It is upon this basis that I would favor five or six main factors in the analysis of personality with possibly a few subdivisions under some of these headings.

If I were asked to make a list of personality traits it would include (1) intelligence, (2) aggressiveness or forcefulness, (3) sociability, (4) personal appearance, and (5) morality. Let us consider each of these in turn. Intelligence, or as we might better say abstract intelligence, the first of this list, might be considered as not essentially or at least completely a social trait. The latter statement is admitted. Yet abstract intelligence is a factor and probably the most important single factor in our social relationships. It is essentially synonymous with "common sense" and presents an important personality characteristic. Aggressiveness is another factor in the relationship between individuals. An aggressive individual is one who dominates any social relationship. An aggressive individual may be either relatively intelligent or unintelligent. It is not always the most intelligent faculty member that dominates a faculty meeting, nor is the successful ward politician always highly intelligent. Sociability, the third trait on our list, is here used in a narrow sense, meaning the interest or lack of interest in people. By pleasing appearance we mean those apparent physical traits which are accepted as favorable or unfavorable by others. While they are admittedly factors, it is more than mere beauty or a matter of dress. In fact very ugly people may have a rather pleasing personal appearance. Lastly it might be difficult to justify morality in such a classification as this. Yet there is a body of laws, customs, and traditions of right conduct which apart from any religious or even abstract ethical viewpoint constitute no small factor in our relationships with others. Conformity to these principles constitutes morality.

Such a classification as has just been presented is admittedly more pragmatic than an attempt to present a complete analysis of human relations. If it is a workable classification of the essential modes in which men are alike or different in their social contacts I am willing to justify it until a better classification is presented.

Before leaving this problem of the list of human traits we should point out another difficulty and that is what is meant and

what is not meant by each trait. We have not yet arrived at a common agreement as to what is meant, for example, by intelligence. Neither are we agreed as to what is meant by the narrower term of abstract intelligence. Similar difficulties arise in defining the other traits of personality. By sociability do we mean a social leader who can get people to follow *without coercion* like Al Smith, or do we mean a leader who succeeds in *compelling* people to follow such as our famous Chicago mayor, or do we mean a person who just likes to be with people rather than alone, or do we mean a person who is most susceptible to crowd influence, as the social butterfly, or will we say that the sociable individual has many friends or not necessarily many but a few very close friends, or lastly how shall we classify the person who is sociable with his superiors or even possibly with his equals but not with his inferiors. Are these six descriptions of one individual or are they descriptions of six different kinds of people? If it be the latter which one will we consider a sociable individual and what shall we say of the other five? Although realizing this difficulty, I am of the opinion that there are ordinarily enough common factors that we may rather generally agree that a person with certain general characteristics is or is not sociable.

I am not in sympathy with the opinion of Trow (14), May, and Hartshorn (11), who find no such traits as confidence or moral judgment and imply that behavior is always specific and that we can never infer what any individual will do in any social situation. I am further aware that Guthrie (6) found little or no correlation between three different tests of introversion-extroversion, but this does not prove that most people are not typically introvertive and others typically extrovertive and the great majority of people located somewhere between these two extremes.

The fifth problem which concerns us is that of the measurement of these personality traits. This is a major problem and one which is undoubtedly receiving more attention than any other in the field of present day psychology. The construction of tests of personality is the most popular psychological pastime of the last half dozen years. June Downey was one of the first who constructed a test for a trait other than that of intelligence. Pressey was another early worker in this field. Since these beginnnings, studies have multiplied until now the bibliography of bibliographies is becoming extensive and among others it includes those of Manson, Roback, Young, Freeman, G. B. Watson and two or three by May. These tests include measurements of everything from facial expres-

sions to morality and religion. If one in a dozen were worth the time and effort required to give it we could have as full a reproduction of personality in quantitative terms as we now have of a modern real estate development. Unfortunately, the problem is not as simple as the measurement of a plot of land and the success is not in direct proportion to the number of tests constructed in the field. Let us enumerate some of the special difficulties encountered in the construction of a test of personality.

One of the most serious difficulties arises in defining the trait to be tested. This difficulty has already been mentioned and the fact that Guthrie (6) found little or no correlation between three popular tests of introversion-extroversion confirms our contention as to its difficulty. Pintner (12) and one of his students recently found the same thing true with two tests of sociability. Even the more widely accepted intelligence tests do not always correlate highly with each other. In fact the correlations often tend to be rather low. Now the causes of these irregularities is either a problem of reliability, validity, definition of terms, or a combination of these. We may have two tests for introversion-extroversion, both relatively reliable measures of what the authors meant by these terms and both valid, but the authors may define extroversion-introversion differently. This problem of defining the traits introduces the problem of what are the traits of personality. Here, as we have seen, there has been much difference of opinion, both in the number of different traits for which tests have been constructed and what is meant by each trait so named.

Another serious difficulty in test construction is that of units of measure. Titchener and Thorndike, many years ago, pointed out that before exact measurement is possible it is necessary to have (1) a starting point, (2) an ending point, and (3) a unit of measure. Let us briefly consider each of these as related to the measurement of personality traits. What is just no sociability or no aggressiveness? These are more difficult problems even than that of zero intelligence. Many attempts have been made to solve these difficulties. The median has been suggested as the beginning point of measurement. Those who hold this view ask why we cannot start with the median rather than the zero point. Thurstone has recently suggested a mathematical method for arriving at an absolute zero point. We can only express a hope that some method will prove successful in the solution of this problem. We have no solution to offer.

A second difficulty is a unit of measure. The just noticeable

difference might be used but it is more difficult to use here than in the field of sensory differences or in that of handwriting scales. Even in these cases we are not agreed as to its feasibility. The standard deviation and probable error have also been used but some unit must be assumed and a median or average determined before we can obtain a standard deviation or probable error. It is true that a variation of 2 sigma means more than a variation of 20 points on an ordinary scale. But all the difficulties inherent in units of measure then arise in the getting of a unit by which to obtain a measure of variability. A third measure somewhat similar in character is reporting scores in percentile ranks rather than in scale units. Another unit which has been used is the age grade unit. Admitting its inherent difficulties the mental age concept in the field of intelligence measurements is less open to criticism than most other units. We are not at all certain and in fact doubt whether a one year mental age unit is the same between three and four years of age as between thirteen and fourteen. Yet mental age is a fairly workable concept and has led to relatively great progress in this field. Unfortunately this idea seemingly cannot be carried over into the other fields of personality traits. The crux of our whole difficulty is to get a unit which is objective and unvariable.

The third difficulty is that of some adequate indication of the present standing of any person in any trait. In measuring the top of a table we may start at one end, use a standard unit, and measure to the other end of the table. In personality measurement the ending point is as difficult to determine as the other two factors in measurement. Here again the work in the field of intelligence tests has gone further than most others. Here there has been a rather deliberate attempt to make an adequate sampling of present attainments. This is not different from the way a chemist assays a piece of metal, a trader grades wheat, an oceanographer studies the temperature and composition of the water and a bacteriologist studies his cultures. This method is satisfactory and successful if the samples are representative. They tend to be representative if we take as large and as varied a set of samples as possible and measure them successfully. Too often in personality traits other than intelligence the tests have been constructed with some notion that a few samples of behavior were sufficient. The sooner we extend our information gained in the field of intelligence tests to the other personality traits the better.

In many of our personality tests we have seemingly used some

arm chair method of arriving at our tests and have not checked up on their representativeness. This, I think, is a valid and serious criticism of some of our most popular personnality tests. In fact we have recently proved this contention statistically on some of the tests. Once this difficulty has been realized it is generally possible to eliminate it by increasing the number and kinds of items and then selecting the best. Do not misunderstand me, the analytical feature in test construction is essential. We desire to see more ingenuity displayed here, but this analysis should be checked up by careful experimental and statistical procedure.

This leads me to a further problem of the criteria by which the validity of any test is to be checked. This question arises in all personality tests. If intelligence is defined as capacity to meet and solve novel situations then what is our criteria for this ability? If we define intelligence as ability to learn we may come closer to an answer. If we are not so concerned with the name of the test and say we want a test which will predict scholarship then we have a criterion. But most of our tests have had nothing more definite than some ratings, generally obtained under more or less difficulty. Franzen and Knight, Clark, Rugg, and others have pointed out the difficulties of rating scales. Manifestly any scheme which uses ratings as a check on the validity of a test is open to serious criticism. It is my honest opinion that many of our tests are better than the criteria by which they were checked. Often some more reliable and objective criterion is available if one is only ingenious enough to use it.

Let me briefly describe a test of introversion-extroversion—you may well question the advisability of the use of this title to the test—which one of our students has constructed and the criterion used in checking its validity. It offers a technique in test construction and validation which warrants careful study and may serve as a model for further test construction.

In the test introversion is defined as that trait which when carried to excess may lead to the psychosis known as dementia praecox and extroversion as that trait which in excess may become manic depressive insanity. The first is manifested by a withdrawal from reality and the second as overindulgence. These are manifestly opposite characteristics. With these concepts in mind a list of one hundred concrete situations or problems were selected in which these opposite types of reaction would come out. Later the list of items was reduced to fifty by working out an index of predictability for each item and selecting the fifty items with highest indexes.

The test was given to the inmates of one of the Illinois institutions for the insane. The diagnosis of each subject was rechecked by a psychiatrist to verify the diagnosis as either dementia praecox or manic-depressive insanity. The tests were given individually but the tester did not know to which group any patient belonged. In all about two hundred cases were tested and the diagnosis of the tests agreed with the diagnosis of the psychiatrist in every case but one.

I have given this test to one hundred fifty college students in a class in beginning psychology and have found almost a perfect curve of distribution. The extremes at either end overlapped with the scores of the two groups from the hospital for the insane. This does not mean that these college students are insane of course. We are now making a study to see if those whose scores fall near the extremes do show definite tendencies in the direction indicated. The first preliminary study shows our suspicions to be well founded. We are yet in process of completing this study.

This study illustrates as we have said a method of test construction and test validation which we hope to extend to other fields. The method of selecting and evaluating test items can be used almost anywhere. Unfortunately we cannot always find any form of exaggerated expression of a trait as the two forms of insanity used in the illustration. Yet I am of the opinion that more such checks can be discovered. Until we can get away from ordinary ratings as a check, and can establish reliable objective criteria, test construction is in a cul de sac.

The last problem to be mentioned is that of the reliability of the test. Much could be said upon this topic but it will not be said here. There is no excuse for a test being published until it has been checked and re-checked for reliability. There is hardly any kind of test which cannot be made to produce relatively reliable measures if the maker takes the trouble so to construct and arrange the test. Anything less than this is unforgivable.

As stated in the beginning it has been our purpose to point out the major problems in personality and the more common difficulties in the construction of personality tests. This we have endeavored to do. We did not promise to give a solution to all of them. In some cases we did point out or propose a solution. Other problems remain without a satisfactory answer. Yet even an appreciation of the problem is a beginning in the right direction. Much loose and much poor work has been done in this field. Many are skeptical not only of the past but also of the future. I admit that rela-

tively few tests outside of the field of intelligence are of great value, but I have hopes that such tests can and will be constructed. Before many years we will have a concise and relatively accurate method of describing a man's outstanding relations to his fellows. This is not a Utopian hope of a solution for vocational guidance but just another step in the evolution of human understanding.

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A THEORETICAL AND STATISTICAL STUDY OF THE PERSONALITY TRAIT ORIGINALITY AS HEREIN DEFINED

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Aside from an abiding interest in those aspects of an individual which are ordinarily subsumed under the term "personality", the experimenter was provoked to attempt this particular investigation because of a summary of experimental literature entitled "Originality", written by Glen U. Cleeton and appearing in the October-December, 1926, issue of the *Journal of Abnormal and Social Psychology*. In a brief but able review of the literature upon this topic, Cleeton indicates the lack of uniformity as to the use of the term and emphasizes the need for definition of terminology.

The experimenter then proposed to make a clear-cut but specific statement defining the term "Originality" and determine whether certain tests could be used to measure that characteristic or quality as defined.

The following procedure was used: On a college campus where the students are very well acquainted with each other, one hundred fifty of the juniors and seniors—all students in psychology—were given a sheet containing the following request:

"Will you please list on the bottom of this sheet, ten persons from the Senior Class and ten persons from the Junior Class whom you consider to be most original according to the definition given below? Please keep the definition in mind and be uninfluenced by anything other than the concept."

The term *Originality* is used to designate the fact that certain persons react to the majority of situations in a novel manner. Their reactions are novel not only in so far as other persons are concerned, but they differ from time to time in the persons who make the so-called original reactions. The term originality as here defined is related to such terms as: constructive imagination, independence, initiative and ingenuity of a constructive sort. It is not to be confused with freakishness nor the tendency to be erratic in a valueless way."

From the returned sheets a list of twenty names—the highest

ten nominated from each class—was chosen. These persons were the subjects used in the investigation. It was necessary to throw out the records of one student, so that the results are actually based upon the records from nineteen students.

Among earlier investigations upon this topic, Chassell, Vol. VII, 1916, of the Journal of Educational Psychology, reports success with a list of tests. From this group three tests—the Chain Puzzle Test, an Analogies Test and an Original Analogies Test—were used in this investigation. In addition, the Kent-Rosanoff list of words for associations was used, and the subjects were ranked by an advanced class in psychology who had experience in the use of rating charts and ranking, and who were given the same criterion of originality as had been used in the selection of the subjects. This ranking was used as a standard against which to check the tests.

The Chain Puzzle Test, the Analogies Test and the Original Analogies Test were scored for time. The Original Analogies Test was also scored for quality. This qualitative score was derived by having an advanced psychology class grade the analogies as to originality without regard to time. The composite grade was then taken as the score. The score for the association test was got by determining the frequency of the individual responses of each subject by referring to the frequency tables established by Kent and Rosanoff, and from the total of these figures finding the average frequency score of each subject. The small score was, of course, given the high rank.

The results of the investigation as they have to date been compiled are based upon the correlations between each test and every other test. Of fifteen such correlations only two of these are large enough to be significant. Between the rankings of the subjects as given by the advanced psychology students and the scores on the Kent-Rosanoff Test there is a positive correlation of .45. No other test correlates with the ranking with a coefficient higher than .25. The indication is that "Originality" as herein defined is susceptible to measurement by the Kent-Rosanoff Test when it is scored in terms of frequency of response, but in so far as this brief investigation has brought to light, the tests chosen from the list recommended by Chassell seem to bear no relation to the term as defined in this investigation. The validity of the use of the rankings as a basis for comparison is dependent upon the acceptance of the use of rankings in any investigation, for the experimenter is satisfied that the work of the advanced class who determined the order of rank

finally was as free from ulterior consideration as is possible for subjective judgments to be. The experimenter is also committed to the belief that in a preliminary investigation such as this the use of rankings is a practical necessity.

Among the tests themselves, exclusive of the students' rankings, one correlation out of a possible ten is significant. Between the Analogies Test, which was scored for time, and the time score for the Original Analogies Test there is a correlation of .55 which, in view of the tests concerned, was probably to be expected. With this exception intercorrelation of the tests is insignificant, which fact itself seems to suggest a lack of common elements among the tests.

The investigation at this time is incomplete, but a few suggestions—some of which are statistical and other theoretical—may be pertinent.

1. The number of subjects is small. More conclusive evidence would undoubtedly appear with increase of numbers. The nature of the trait measured necessarily limits the number of individuals who may be so measured. Originality—however defined—is a *rara avis*.

2. The fact that the tests used successfully by Chassell were not discriminative in this investigation may be due to the manner in which our term was defined.

3. From incidental observations as well as from the fact that the results are largely negative, the experimenter is inclined to believe that a delimitation of the term will serve a better use in scientific terminology than so broad a term as was herein defined. Certain of the subjects—one of whom was elected for the college annual as the most original person in college and another of whom writes very cleverly for the college magazine—but both of whom are lacking in mathematical proficiency—made very poor records on the Chain Puzzle Test, which is similar in nature to the Ingenuity Test which appears in the Stanford Revision of the Binet-Simon Test. Such observations open the possibility of a clear-cut difference between originality as applied to literary invention and as applied to ingenuity.

4. The concept as herein defined involves a broad usage. That this definition should be made narrower seems apparent. Such information is important for further investigations, some of which are in the process of being made, and others of which are under consideration by the experimenter.

5. A frequency table for the responses of unselected college students is being compiled by the experimenter. Such a table may alter the scores derived from the Kent-Rosanoff table as used in this investigation, and they will also furnish a basis of comparison for curves of distribution when sufficient records can be got from persons who are legitimately selected as being original.

6. This paper is intended to be suggestive and not conclusive in nature.

AUTOMATIC WRITING BY A BLIND SUBJECT*

By SARAH M. RITTER
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THE charts to be shown with this paper present reproductions of selected specimens from a series of automatic writings done in the summer and autumn of 1920.

The subject, to be known as E—, was a man sixty-five years of age, stone blind from the age of two and one-half years. Professionally, E— was a musician, distinguished in two continents as a concert pianist, composer, and author of works on musical interpretation and criticism. He also held various college positions as teacher of piano and lecturer on aesthetics. He was acquainted with much of the best in literature, as well as in music, and in some of the other arts. The science which had been given him was largely of the popular or *pseudo*-sort.

E— claimed to have no knowledge of the alphabet save only a partial list of the letters which he made by conscious effort (see line one of Plate VIII) in a form very different in most instances from that used in the automatic script. His writings for publication and most of his correspondence had been dictated to an amanuensis. Intimate letters written by his own hand had been in a Braille resembling not at all the forms of script or print.

In 1917, Mrs. E— (or "Nettie") had passed away. With her going, E— said, the "light" of his personal and professional life went out. He suffered a physical and something of a mental collapse, following that event, but rallied and was able to continue his work, successfully, during the remaining seven years of his life. To aid in his recovery friends had sought—not in vain—for mediumistic messages from "Nettie". E— was assured that Mrs. E— was often near him and would in time "learn" to manifest herself to him in an "evidential manner".

Such assurance had come with peculiar impressiveness in early June of 1920, when E—, enroute to his summer home (a home full of memories of "Nettie") had stopped in Boston and paid a visit to a noted medium there. A little later the summer days were being spent by E— and a group of house guests in strenuous practice for coming concerts. The evenings were occupied in the reading of

*Abstract of a paper read at the Columbus meeting of the American Psychological Association, Dec. 29, 1927.

post-war literature of the Sir-Oliver-Lodge-Sir-Conan-Doyle type. At last E— was given a pencil and paper and asked to assume a passive attitude for the reception of possible "messages". Almost immediately the pencil began to move, mechanically, across the paper. Evening after evening, from July 13 to about the middle of August, the experiment continued, two sheets of letter paper being covered at a sitting. These papers were numbered, dated, interpreted—even merrily criticized—by E—'s close friend and guest, a Mrs. D., *B.* (Typical selections are shown in Plates I-VII.)

The series was brought to a close when an "unfriendly spirit"* seemed to gain control of the pencil. Thereafter E— claimed to have lost all *rapport* with the "source" of his writings, and to be unable to do more in the automatic line. However, in late September, to oblige the writer of this article, E— made another attempt. (The results are shown in Plate VIII.)

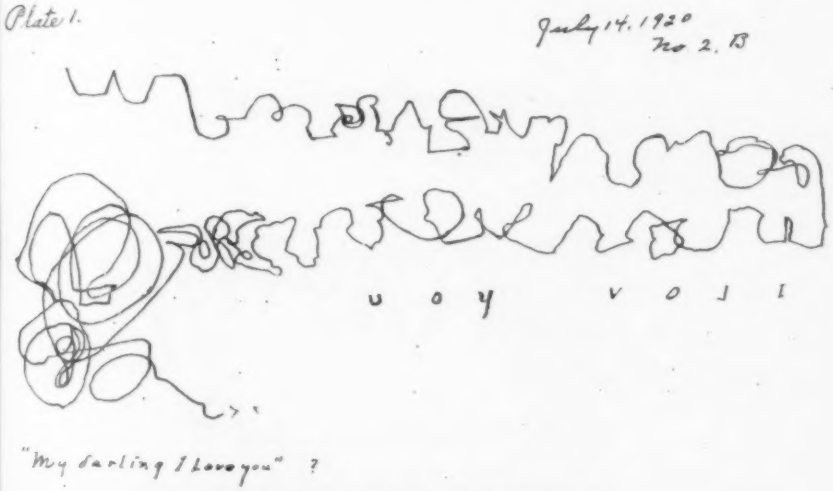
At this last sitting the first request was for a list of such letters of the alphabet as E— thought he knew; for he had said, "I never learned to write, and do not know the form of the written letters, though I do know some of the printed letters made square, as in my signature, for instance." While complying with the request E— named the letters as he made them, mentioning the following as those of which he had "not the remotest idea": *G, J, K, Q, X, Z.* The *G* appears frequently in the automatic writings, the *K* at least once. When asked if in ordinary writing he would make *A* as just demonstrated, he said, "Yes; *A* is a square, closed up, with a little something out to the right □." Asked if as a child he might not have played with blocks having raised letters, he replied, "Very possibly I did." E—'s method of producing the line of letters shown was by grasping the pencil in the fingers of both hands and measuring his way cautiously across the page. When this was done, and a fresh page provided, he held the pencil lightly in the fingers of his right hand only, relaxed the tension of the preceding effort, and *waited*—for the automatic demonstration. Nothing happened. Then a relative of E—, present at the interview, suggested that another person in the room put the tips of her fingers about the top of E—'s pencil, lightly touching it. This was done. Immediately the pencil wavered; then moved, ouija-like across the page. At the completion of the first few letters the helping fingers were removed, but the pencil continued, forth and back, until the

* This "ghost", according to E—, had appeared to him many times and menacingly, throughout his life, and especially in his youthful hours of piano practice. Concerning him E— once consulted William James. The ghost was finally "laid"—after the events reported in this paper. But that is "another story".

final ending in meaningless curves. At this point, evidencing an awareness of at least a *difference* in the output, E— remarked, as to a person palpably present by his side, "This doesn't mean anything, dearie." And the experiment ended.

EXPLANATION OF THE PLATES

With each of the reproduced writings there is appended or interlined a surmised interpretation. The first evening's work is not shown, as it had resulted in nothing but scrolls and flourishes, interlaced and recrossed in a manner impossible to decipher or to copy.



* Continued in a dim and hopeless scrawl.

Plate I shows E—'s second performance. Already he had learned to separate his lines, doing very little overlapping except at the end. A few letters also stand out so distinctly that it is possible to guess, in the light of what followed, at the meaning of the whole.

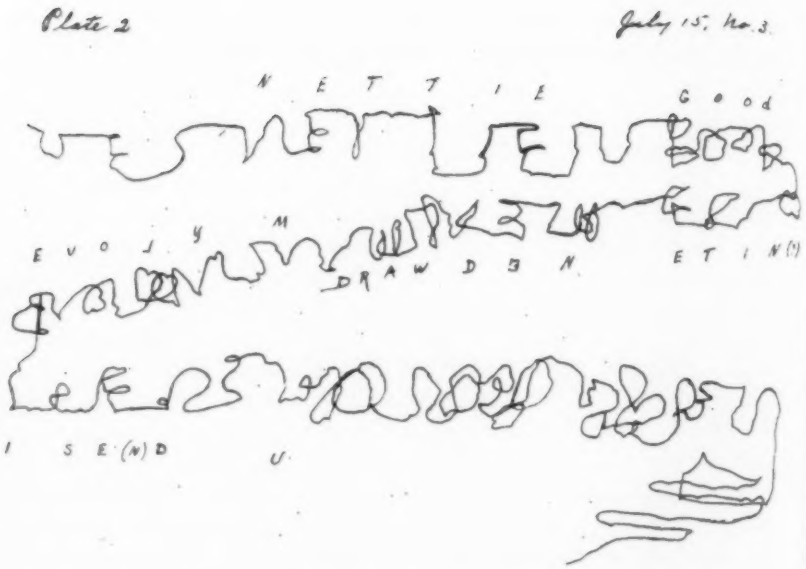


Plate II. On the third evening E— still writes both ways across the paper, and loses himself finally in a scrawl. The word "Nedward" is his own pet name, known to Mrs. E—. The *U*'s, *V*, *Y*, and *A* are much alike.

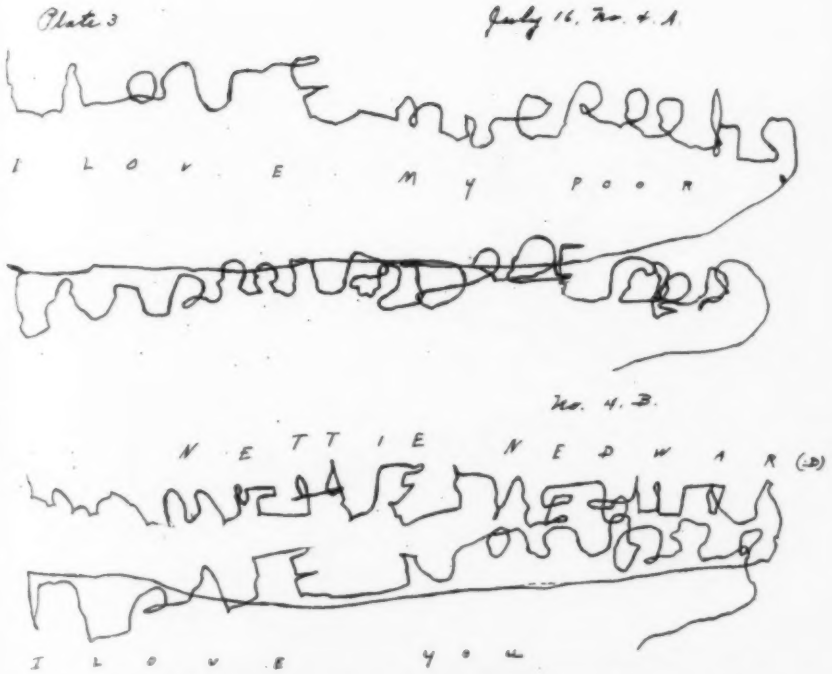
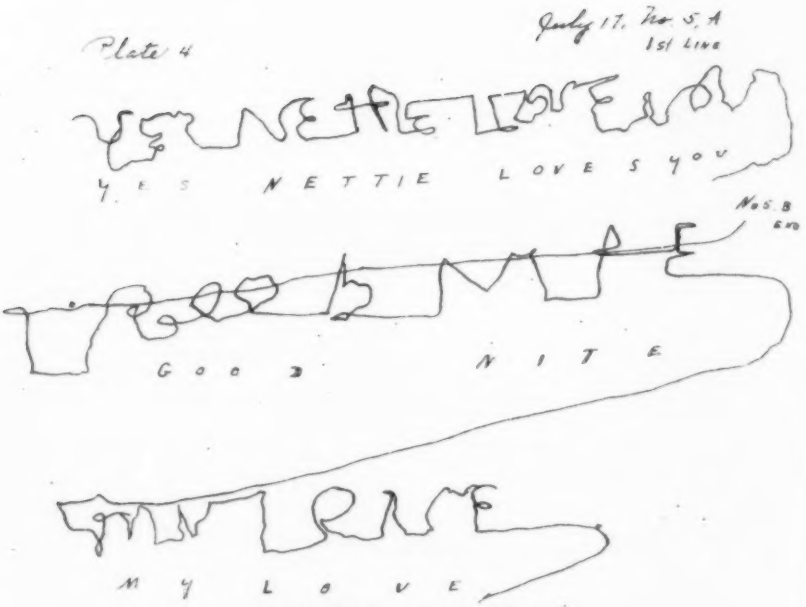


Plate III. At the fourth sitting E— had learned to swing across to the left for the beginning of a new line. The *t*'s are better crossed. The *A* is still inverted, but shows some evidence of a cross.



Another line of faint scrawls.

Plate IV. **At the fifth trial there is great gain in legibility.**

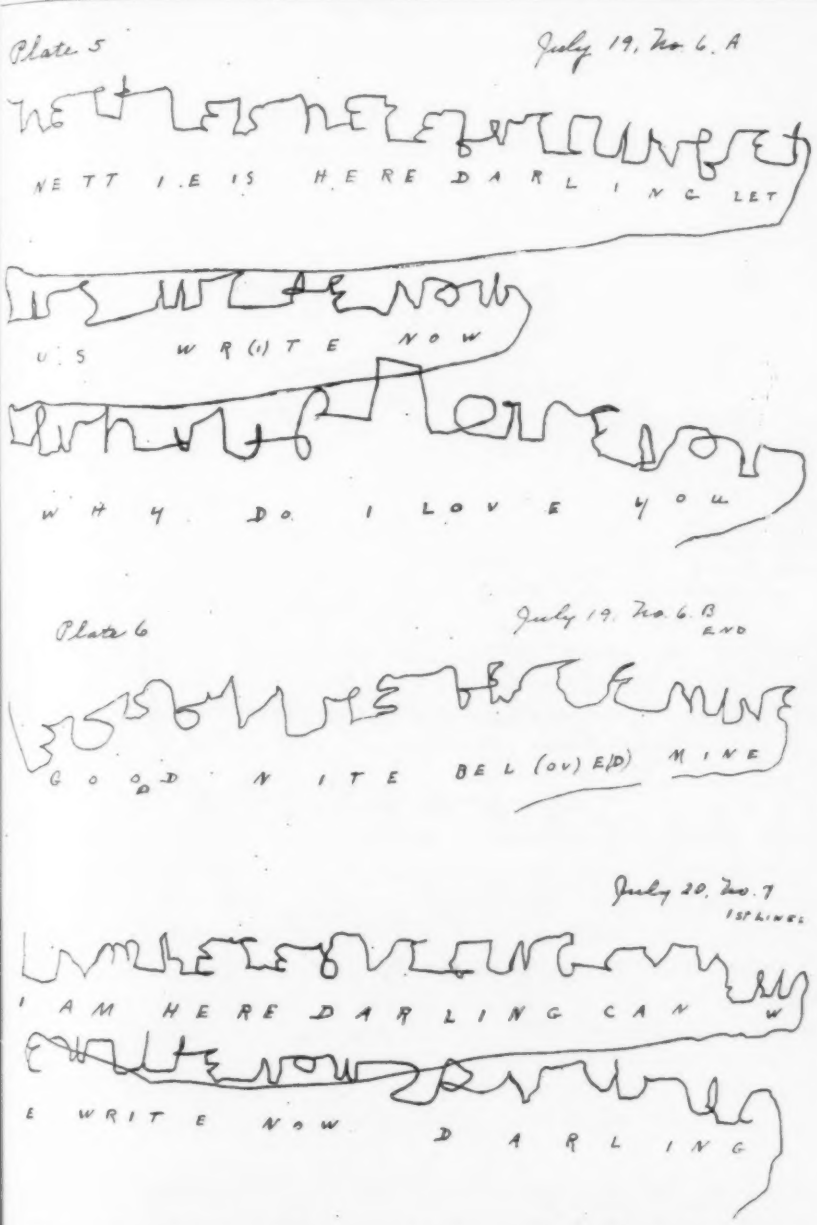


Plate 5
July 19, No. 6. A
NETT I.E IS HERE DARLING LET
U S WRITE NOW
WHY DO I LOVE YOU
Plate 6
July 19. No. 6. B END
GOOD NITE BEL (OV) E(D) MINE
July 20. No. 7
1ST LINE:
I AM HERE DARLING CAN W
E WRITE NOW DARLING

Plates V and *VI* exhibit merely the continuation of the writing, with little progress as to clarity of script or of "message".

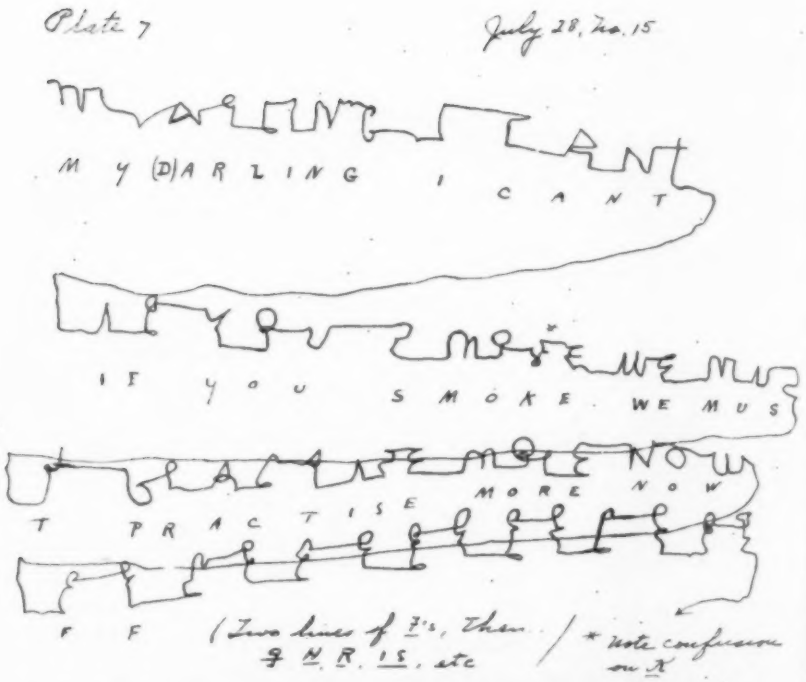


Plate 7
July 28, No. 15
MY DARLING I CANT
IF YOU SMOKE WE MUS
T PRACTISE MORE NOW
F F (Two lines of F's, then
F N R IS, etc
* Note confusion on X

Plate VII. **Skipping to the fifteenth sitting, we find the *A*'s right side up, the *Y*'s improved, the *K* showing confusion but invention as to form. The idea of practice is introduced.**

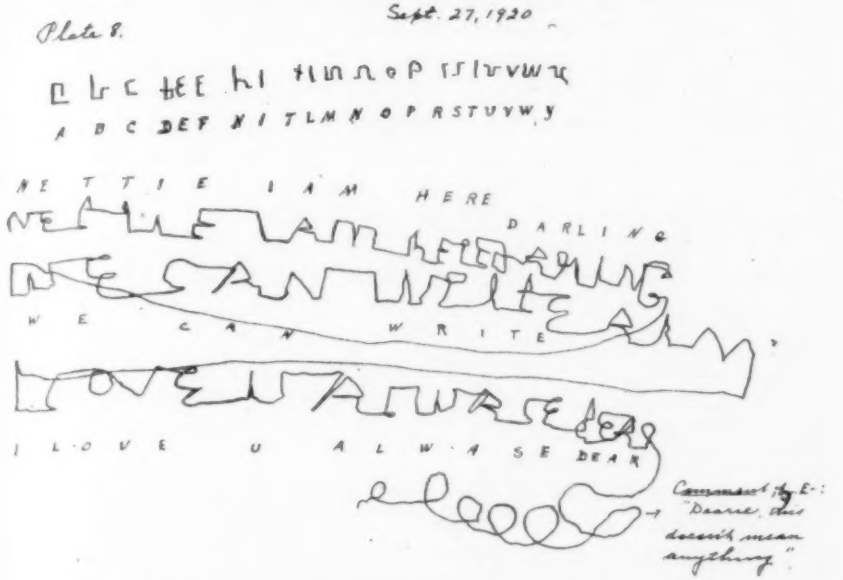


Plate 8.
Sept. 27, 1920
A B C DEF N I T L M N O P R S T U V W y
N E T T I E I A M H E R E
D A R L I N G
W E C A N W R I T E
I L O V E U A L W A S E D E A R
Comment by E—: "Dearie, this doesn't mean anything."

Plate VIII has been discussed. At the top is the alphabet as E— thought he remembered it. Below is the last of his automatic writings. Note the surprising perfection of his *A*'s, and certain other letters; also the quaint spelling of "ALWASE".

SUBJECTIVE ATTITUDE OF E—

Evidence of some "inner speech", or awareness of content, was found in the manner of terminating the last paper (Plate VIII). Again, in referring to a sentence in Plate VII, "My darling, I can't if you smoke." E— added, naïvely, "And I immediately laid down my cigar." Another paper, not reproduced here, contained the sentence, "My darling, do not bear so hard on the pencil." This was followed by several lines of very much lighter strokes. Doubtless in each instance the recognition of the content came with the same type of surprise that has accompanied other well-known cases of automatic and ouija writing—or the dénouement of our dreams.

The movement of E—'s hand, in the writing, had the smooth glide of the ouija pointer. It was found, by experiment, that the hand or index finger, not only of E— but of each of several normal but blinded-folded subject, could, when "passively yielded", be substituted quite effectively for the pointer on the ouija board. The

subjects, of course, wrote (muscle reading fashion) only what was in the "minds" of the guides. Dr. June Downey has shown (see *The Will-Temperament and its Testing*) that a hand so yielded can be induced to write, with a pencil, what is in the mind of the subject. This subject is aware at the moment of what is being written, but is unaware that he is the originator of the movement also. This latter must have been E—'s state when the presumably guiding hand was likewise a "figment of the imagination".

The subjective origin of this work—characters, content, and movement—must have been suspected at last by E himself, else he would not so readily have given up the fascinating pursuit. If further evidence to the point were necessary, it could be added that, so far as "we on this mortal plane" are able to judge, the cultured Mrs. E—, had she been the author, would have chosen, for so willing a hand, a type of script very different and a content of thought more significant.

The suggestibility of E— (for auto- or hetero-impressions) was augmented by the fatigue of the evening hour, a tendency to depression, the concentration of attention within narrow lines, and the "nearness" of the paraconsciousness with its pressing demands. The "messages", apparently, are of a wish-fulfilling character. They "are such stuff as dreams are made on."

BEHAVIORISM, ETHICS, AND PROFESSOR WEISS

By W. H. ROBERTS
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BEHAVIORISM and ethics just will not mix. Bertrand Russell pointed this out in a characteristic essay in the Century Magazine for December, 1926. Notwithstanding expert advice, Professor A. P. Weiss has attempted the impossible in THE JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY for March, 1928. The only result is that he exhibits the poverty of behaviorist categories, the clumsiness of behaviorist reasoning, and the impossibility of founding any ethics at all on such a metaphysics. It is just such a result as follows when, without due precautions, one mixes the two portions of a seidlitz powder. The fizzing, foaming liquid will not stay in the inadequate vessel provided for it but spills over in every direction.

Mr. Russell pointed out the very simple reason why behaviorism could never provide a foundation for ethics. The latter assumes that certain actions or experiences are *to be desired*. It undertakes to explain why they should be desired and why men should exert themselves to perform or enjoy them. But according to behaviorism man never acts *for* anything. None of us went to dining room or kitchen today because we wanted something to eat. (See Allport, "Social Psychology", p. 2.) Professor Weiss did not write his essay with any end in mind. To admit either a purpose or a mind is to be "unscientific".

And since men can never *want* anything, there can be no such things as values. It is simply impossible to discuss the question whether one line of conduct is *better* than another or to be preferred to another. There is no *reason* whatever to save money for old age, to be kindly to our neighbors, or to preach the gospel of behaviorism.

Of course behaviorists are seldom as silly as all this. Every once in a while they forget they are behaviorists and talk sense. The point is that to be sensible on such points they have to abandon behaviorism.

The most determined attempt to carry behavioristic determinism and indifferentism to its logical conclusions was made by Spinoza. Neither Professor Weiss nor any other behaviorists have added anything essential to his arguments. (The principle of the "con-

ditioned response", *e.g.*, is clearly expounded in the "Ethics".) Life, however, was too rich and potent even for Spinoza. In the end he called upon man to choose between slavery to sense and passion and the freedom of the "intellectual love of God".

Professor Weiss is not so consistent as Spinoza. After asserting that all human and social activities are only highly complex integrations of electrons moving according to the law of opposite signs, he comforts himself and his readers with the assurance that, once this truth is grasped, "man's behavior will become more coöperative, more considerate of others, more diversified, more artistic, more useful". The interesting possibility may be granted—but why should anyone care? Why should Professor Weiss write essays and books to hasten the coming of such a millennium? And what action can he expect from readers who as merely physical systems are insensible to discussion, to be reached only by *con*cussions?

"What do I care what becomes of a sabre toothed tiger?" exclaimed a Cro Magnon man when informed that his mother-in-law was fighting with such a beast. What do *we* care whether electrons whirl in one configuration or another?

But of course we are mistaken in supposing that we *care*. We only think we care. No, that is a delusion, too, probably due to the baleful influence of religion or "sentiment". (How mere electronic aggregates can be the victims of "sentiments" is a delicate problem.) We only think that we think that we care. We cannot rest even there. The chain of erroneous "thinks" must stretch back to infinity. Anyhow, we don't *think* at all, nor care. We only move.

Hedonistic, or pleasure-pain, theories of human behavior, Professor Weiss assures us, are unscientific because they assume that pleasantness and unpleasantness are mental states which have no physical properties but nevertheless act on the sensorimotor system. Even a child can see the contradiction involved. Ethical thinkers, not being children but wise and acute adults, have never committed themselves to such absurd metaphysics. If someone alleges Descartes, I would reply that Descartes was a mathematician and physicist and that philosophers ever since his day have repudiated his impossible dualism.

The ethical theorists have been content to stand on the fact that human beings act for ends. How they *can* do so, is not an ethical problem. It is raised, entirely properly, by metaphysical psychologists. But it is only the hasty, impatient, and therefore

crude, metaphysicians among them who invent the contradictions they so triumphantly expose.

There is a famous story of some clever impostors, who persuaded a king and queen and an entire court, without any foundation in fact, that they were dressed in garments of surpassing loveliness. The delusion lasted until a little child of the common people exclaimed, "Why, they're all naked!" It certainly seems to be time for "unscientific" common sense to protest that there is something wrong with a psychology that reduces purposeful action to a delusion. Whatever Professor Weiss may say, I am confident that he wrote his article *for* a purpose, to aid in securing an end that he *valued*.

As the Chinese belabor their gods when the divinities do not send the proper amount of rain, so with characteristic behaviorist consistency Professor Weiss first deifies and then reproaches the physicists. Physics, he tells us, is the science that is best qualified to answer questions as to the *ultimate* structure and functions of the universe. But when such writers as Whitehead and Millikan question the adequacy of physical categories for an explanation of the universe, Professor Weiss assures us that the reason for their pusillanimous attitude is that they have not studied human affairs! To one in search of logical consistency it would seem that (1) human affairs are in some way "out of" the universe or that (2) a science which ignores them cannot be more than a partial account of reality, a perspective from a single point of view. Physicists, as a matter of fact, seem to feel some such misgivings as to their science. It is curious that psychologists are obliged to reassure them!

The abstractions with which physics deals are curiously like the naked royalties and near royalties of the old fable. They have been stripped of all the habiliments of majesty. But as society is an order in which kings are something more than naked bipeds, so the universe is not only one in which planets arrive at appointed positions on schedule time or chemicals behave in predictable fashion in test tubes. It is also a universe in which men labor for distant ends, love, and play, and worship. The one group of facts is as characteristic and significant as the other. But only an impatient metaphysician supposes that their relation in a system amounts to a reduction of one to the other.

Professor Weiss, however, becomes considerably more moderate toward the latter part of his essay. "Behavioristic ethics", he assures us, "merely affirms that any program of social control

that is based on science will be more enduring than one based upon tradition and sentiment. The newer idealism will be one that has a scientific background." But which of the ethical thinkers ever thought anything else? Which of them did not labor with all his might to substitute sense for sentiment, science for tradition? Socrates, Plato, Aristotle, Epicurus, Epictetus, the Buddha, Mill, Moore, Dewey—which of all these has not struggled to do exactly what the behaviorists propose to do? If any of them have stood for obscurantism or irrationality, I at least have failed to detect it.

One more instance of Professor Weiss's inconsistency should be noted. After affirming that every event is a function of all the events that have gone before, he calmly asserts that responsibility will vanish when we understand the real causes of human action. But do not actions have their consequences? Are not subsequent events "functions" of the decisions any individual makes? And is not this the very responsibility that Professor Weiss denies? Certainly I can think of no more poignant realization of responsibility than just that knowledge of a "functional" relationship between one's actions and their results.

A SOCIAL ASPECT OF BEHAVIORISM

By WILLIAM D. TAIT

BEHAVIORISM is a new school of psychology or if you prefer a school of new psychology. One of its chief exponents, Dr. J. B. Watson, has in a sense brought us back to Locke and his *tabula rasa,* but he has outdone Locke for he has left us with little or no inheritance. "Silk purses can now be made out of sows' ears" in this modern factory. There is not much importance attached to what McDougall means by inherited inner drives. The organism is to be considered as the fortunate or unfortunate recipient of whatever the environment may bring to it. Children with defective ancestry are potentially quite as capable as children with effective ancestry. Dr. Watson has said this in no uncertain terms, although his explanation about the variations among children in the same family is somewhat thin.

However, the chief interest here is neither Watson nor McDougall, but what might be called the social consequence of behaviorism. A great many people believe that society can be improved by the process of changing human nature, not by gradual selection, but by the rapid process of providing a certain kind of environment during the present generation. At the present time, this idea is running riot as one can see by the plethora of social reform legislation. One of the arguments of the prohibitionist is that if alcohol is forbidden to this generation, then the next will not desire it. Surely this is asking too much. Curiously enough this is apparently one of the behavioristic principles which Bertrand Russell accepts yet he rejects many of its main theses.

Here, as elsewhere, the matter of definition is important. If we mean by human nature just the way in which people act as if that were all, if we mean just behavior explicit or implicit and no more, and exclude all drives or impulses then of course we must admit without further argument that human nature in that sense can be changed and changed radically—for better or for worse. The point of course arises as to who will decide as to better or worse and upon what factors the judgment will be based. If then, environment accounts for the greater part of human actions it should loom large in any scheme or schemes aiming at social betterment, educational, moral, artistic or otherwise. It might mean, too, that there would be some rapid changing on the

stage of life. Comedy and tragedy would alternate with bewildering rapidity for there would be no inner guiding principles or springs of action.

Those who believe, as they say, in human salvation find themselves in the same bed as the behaviourists because of the emphasis with which they stress the potency of environment to reconstruct human kind and remake the face of things. But if human nature is more than behavior in the narrow sense, if there are drives, call them as you will, instinctive or otherwise, which are part of the inherited structure, which are determiners in a very real sense, then it may not be incorrect to say that human nature cannot be changed to any great extent or degree. The inherited structure of each of us sets its irrevocable limitations. Human nature will go on its way just as all nature does, irrespective of what we think *of* it or *about* it. No one denies that the forms in which the drives manifest themselves may be modified but that is not at all the same thing as changing the drives themselves. It is therefore a logical consequence of behaviorism, for logic is necessary in psychology as elsewhere, that human nature can be reconstructed. When they awake, it will be interesting to note how the environmentalists connected with the salvaging of human wreckage will accept this necessary deduction from their attitude. They and the behaviorists are at one on the question of redemption. Change a man's responses and you change all is a plainer way of stating the case.

It is doubtful if there are many who will admit, that with all their efforts, the inner and original impulses of man can be modified as the result of modifying the expressions of these same impulses. If then, nothing but the expressions can be changed it follows either that human nature cannot be made over or else the behaviorists are correct. Social workers, preachers, missionaries, reformers and all that class are placed on the horns of a dilemma. If they insist that human nature can be turned inside out like an old garment then they and the behaviorists of the extreme school are at one. If they rebel against this, then they must accept the company of those who believe that human nature cannot be made over by the waving of a wand. It is doubtful if our social prophets have realized their place in the behavioristic universe. Bertrand Russell, in this respect, is difficult to understand. He appears to accept behaviorism with one hand and reject it with another. In his book "Education and the Good Life" he makes free use behaviorism yet in his

"Philosophy" its essentials are adversely criticised. If the essentials are wrong, how can the application be right?

Carried to the extreme, the principles announced by Watson will solve all the problems connected with vocational guidance. At present, we are trying to measure innate capacity as well as we are able, but if behaviorism is right then such a procedure will not be necessary. All that will be neeessary will be to train the child in any vocation selected at random and he will become proficient if the training is efficient. If the child does not attain to a standard then it is the education and training which are to be blamed not the child. It is beautifully simple but does it work or will it work?

Behaviorism, then, should have a sweeping victory with that class of sanguine optimists who believe in rapid advancement towards betterment by the simple expedient of making adjustments in the externals. The League of Nations should become a branch of behaviorism. Behaviorism should be the psychological bible of the reformers, the educational faddists, the social and international uplifters, feminists and all who believe that some one or something else than himself can save the soul of a man. If the soul is the soul of a behaviorist, the matter is simple for the burden is light.

REVIEWS

THE PSYCHOLOGY OF CHARACTER, WITH A SURVEY OF TEMPERAMENT. By A. A. Roback. New York, Harcourt, Brace and Co., 1927. Pp. xxiv+595.

This is a monumental work on character and temperament. It is comprehensive and erudite but at the same time interesting and stimulating to thought. It consists of four parts. Part I, Historical, includes discussion of the literary characterologists from Theophrastus to modern times, the course of the humoral doctrine, contemporary views, the applied psychology of temperament, character in proverbial lore and inspirational literature, and the critical approach to the study of character by Bacon, Mill, and others. It ends with a chapter on the definition of the terms personality, temperament, and character.

Part II is entitled "Classification of Characters". It contains the following chapters: Fourier's Socio-Economic Treatment of Character, The British Writers, The French School, The Teutonic Schools, and Other Continental Schools. In Part III "Movements and Methods" are discussed under the following headings: Suggestions from Psychiatry, The Psychoanalytic Approach, Compensation as a Fundamental Mechanism in Personality, Through the Perspective of the "Struktur" Psychology, The Endocrinological Attack, The Behavioristic Detour, The American Experimental Contribution, The Philosophy of Character, and Biographical and Historical Material as Sources of Character Study. This part ends with a discussion of the interrelation of the various methods which are conveniently arranged and presented on a chart.

Part IV is entitled "Constructive". In this part, consisting of ten chapters and approximately 125 pages, the author expounds his own point of view, *rational inhibitionism*. In the first chapter of this part character is defined as "An enduring psychophysical disposition to inhibit instinctive impulses in accordance with a regulative principle". The remainder of the book is an expansion and defense of this thesis. It is shown to imply (1) instinctive impulses, (2) inhibition, (3) consistency, which is described as a tendency to universalize behavior and is thus suggestive of the Kantian categorical imperative, (4) intellect, and (5) regulative principles, which are arranged in the following hierarchy: physical, legal, social, religious, aesthetic, and ethico-logical. Character for the author is preëminently an inborn quality of the personality; for the power of inhibition, the urge to consistency, and the basis of intellect are all innate factors.

Roback's theory of character is interesting and suggestive, but the reviewer feels that it will not be readily accepted by present day psychologists. A detailed discussion of the numerous controversial statements in the text will not be undertaken, but a few general comments may be made. (1) The concept of character is perhaps too narrow. It might be broad-

ened so as to apply to the total conative aspect of the personality, just as temperament includes the total affective aspect. It would thus include the number and intensity of the impulses (whether instinctive or habitual) and their interrelation and organization as well as the power of inhibiting them. (2) The nature of inhibition is not made clear; and, moreover, inhibition is probably not inherited, as the author suggests. There is good evidence from genetic psychology for regarding the particular kind of inhibition involved in character as "habits of control" which are acquired separately for each specific impulse and situation. (3) Environmental influences are not given sufficient weight. They may be of more importance than innate factors in the formation of character. They probably result in the production of some if not all of the impulses, in the formation of habits of control, and in the development of the urge for consistency. When the author argues that the last mentioned is innate, one feels that the "flight from reality" is complete and that it is time to return to the observation of developing human beings in a real twentieth century world. (4) The arrangement of regulative principles into a hierarchy gives an ethical turn to the discussion. In fact the author never quite succeeds in separating the ethics from the psychology of character; and furthermore, his psychology seems to be determined very largely by his ethical views. (5) These ethical views are also open to criticism. They are traditional and conservative if not patriarchal. There is something rigid and inflexible about moral character as here described. The ethical views are thus a bit "out of joint with the times". They are the views of one whose motto would be "Odi vulgus profanum et arceo".

Notwithstanding the above criticisms, the reviewer feels that the author has rendered a distinct service to psychology by his comprehensive survey of the literature and the methods in the study of character and temperament. All the different approaches to the subject are well presented. The chapters on character in literature, folk-lore, and biography are particularly interesting and deserve special mention. The classification of methods will be of great assistance to the investigator and will no doubt stimulate further research in this field. The book will be found exceedingly useful in advanced courses or seminars on this subject. In this connection his "Bibliography of Character and Temperament" published separately will be found an invaluable reference work. The students of character will feel greatly indebted to Roback, for by his arduous and painstaking labor their task has been considerably lightened.

J. W. Bridges.

Religion in the Making. Lowell Lectures, 1926. By Alfred North Whitehead, Fellow of Trinity College in the University of Cambridge and Professor of Philosophy in Harvard University. New York, The Macmillan Company, 1926.

Professor John Dewey characterized "Science and the Modern World", whose direct sequel the present work is, as "the most important restatement,

for the general reader, of the present relations of science; philosophy, and the issues of life, that has yet appeared''. This doubtless has served even to increase the number of readers of the religious philosophy of Mr. Bertrand Russell's distinguished collaborator in ''Principia Mathematica'', of whom it was remarked at the recent International Congress of Philosophy that he is saying ''perhaps the most fundamental and the most challenging things that are being said today''. A clear theism is here established not at all upon epistemological considerations, but directly upon analysis of the natural world of ''becoming'' itself, God being shown rigorously necessary for the complete analysis of the concrete ''occasion''. In other words, although not, as La Place's famous reply to Napoleon with such relative measure of right maintained, an ''hypothesis'' necessary for work interior to the physical sciences, God is directly relevant no less assuredly to the philosophical issue directly confronting the physical scientist himself today, of making a true analysis of the universal characters of nature. The theism which is thus presented directly as a necessary constituent of any coherent scheme of physical thought stands in a contrast with the ever pantheizing quasi-theisms of idealistic thinkers, precisely for the reason that little or no attention is paid by Dr. Whitehead in this connection to the psychology of knowing. This feature, apparent in his earlier volumes, as Professor A. E. Taylor has remarked of him, ''has been noted for a defect by some of his critics, whose own interests are more in psychology than in physics, but the fault, if it is a fault, is more than redeemed by the consequent freedom of the theism of 'Science and the Modern World' from all suspicion of being a fanciful personification of a human cognitive function''—not to speak of the fallacies incident to mistaking psychology for epistemology.

In the first of these succeeding lectures we meet at once an incisive affirmation which is avowedly ''the direct negation of the theory that religion is primarily a social fact'' (though duly guarded with reference to the importance of its social facts). ''Religion is the art and theory of the internal life of man'' (which is ''the self-realization of existence'') ''so far as it depends on the man himself and on what is permanent in the nature of things''. ''It is the transition from God the void to God the enemy and from God the enemy to God the companion.'' And ''religion is force of belief cleansing the inward parts''. Religion having emerged, as to its external expression, under the four successively developing aspects of ''ritual, emotion, belief and rationalization'', ''its pre-rational type is ceremonial, mythical and sociable'', and ''when a modern religion forgets the note of solitariness—'strait is the gate and narrow is the way, and few there be that find it'—it is suffering from an atavistic relapse into primitive barbarism. It is appealing to the psychology of the herd, away from the intuitions of the few''. Yet religion's concepts ''claim universal validity''. ''Its doctrines aim at being that metaphysics which can be derived from the supernormal experience of mankind in its moments of finest insight.'' And ''reason mocks at majorities''. An admirable (though at points not unchallengeable) brief survey of religious history

discloses that "the coming of rational religion is the consequence of the growth of a world-consciousness". It concludes: "Today the two catholic religions of civilization are Christianity and Buddhism, and—if we are to judge by the comparison of their position now with what it has been—both of them are in decay. They have lost their ancient hold upon the world."

In the second lecture, captioned "Religion and Dogma", we presently find the author continuing this comparison of Christianity and Buddhism, and it is a recurring *motif* of the book. One cannot indeed but wonder whether he fully bears in mind the distinction between the Buddhism of Buddhist history and the classical Buddhism, and the grounds upon which it has been possible for authorities to refuse to classify the latter as a religion. Is the psychologist Thouless, of Manchester University, not right when in his recent "Social Psychology" he writes: "It is a great mistake to regard the Buddha as the founder of a religion. He regarded his mission to men as rather to save them from religion, and to lead them to a correct world-adaptation by understanding and controlling their own mental processes. . . . He would have his disciples find a satisfactory world-adaptation not by religion but by practical psychology. The later exaltation of this essentially anti-religious teacher to a position in the Godhead worshipped by the greater number of . . . modern Buddhists is an illustration of the incurable preference of most men for the religious adjustment." Dr. Whitehead himself proceeds with a series of most effective contrasts: "Buddhism is the most colossal example in history of applied metaphysics. Christianity . . . has always been a religion seeking a metaphysic, in contrast to . . . a metaphysic generating a religion"; and "consequently has retained the easy power of development". "The Gospels exhibit a tremendous fact. The Buddha left a tremendous doctrine. . . . The Buddha gave his doctrine to enlighten the world. Christ gave his life. It is for Christians to discern the doctrine." "In respect to its treatment of evil Christianity is therefore less clear in its metaphysical ideas, but more inclusive of the facts." "Buddhism makes itself probable by referring to its metaphysical theory. Christianity makes itself probable by referring to supreme religious moments in history."

Christ's own reported sayings "are actions and not adjustments of concepts. He speaks in the lowest abstractions that language is capable of if it is to be language at all and not the fact itself." As regards Paul, Dr. Whitehead seems to the present reviewer distinctly lacking in insight, as compared with his expressions upon Christ and upon John. One cannot but sympathize with him in view of the misunderstanding Paul suffers through the overlaying of his religious spontaneity with the constructions of Protestant scholasticism. Paul has, however, a highly distinctive core of intuition which is just what would supply an ultimate possible lack in Dr. Whitehead's religious thought. It may be an accident, but again may not, that the word "sin" (though in a work on "religion in the making") not once occurs, and that the word "evil", so constantly occurring, is never

defined. The Pauline features which are relevant here are only *sublimated* in the Johannine type of thought, in that era of classical Christian origins. And according to Dr. Whitehead "the modern world" which "has lost God and is seeking him", "must find him with the help of John". But he adds "and not of Paul". The matter is not unrelated to certain challengeable features of his difficult (though welcome, and important) metaphysics.

As to "religious experience in its most general character", the spirit merges its individual claim upon the attainment of life with that of the objective universe—for "religion is world-loyalty"—at once surrendering and appropriating. This "formative principle of religion" involves "a revelation of character apprehended as we apprehend the characters of our friends". This is, however, to Dr. Whitehead, not "a direct intuition of God as a Personality substrate to the universe" (though the latter "may be a legitimate inference from it"). It is "a character of permanent rightness whose inherence in the nature of things modifies both efficient and final cause, so that the one conforms to harmonious conditions, and the other contrasts itself with a harmonious ideal". The recognition of this as the ultimate nature of direct religious experience is regarded as "very fundamental to the religious situation of the modern world", for "the psychological interpretation (is) fatal to evidence unable to maintain itself at all emotional temperatures amid great variety of environments". But "reason is the safeguard of the objectivity of religion. It secures for it the general coherence denied to hysteria."

"Today there is but one dogma in debate: What do you mean by 'God'?" Distinguishing the three main concepts as the Eastern Asiatic (impersonal self-ordering of the world), the Semitic, and the pantheistic, Dr. Whitehead remarks that Christianity adopted neither one, "true to its genius for keeping its metaphysics subordinate to the religious facts to which it appeals". He refers first to Christ's own modifications of the inherited Semitic concept, especially in "the concept of God under the metaphor of a Father" and in "the association of God with the Kingdom of Heaven", and that as "within you". (This is, however, a widely current mistranslation. The context requires "among you".) In closest connection with this he passes to "the introduction of the Logos-doctrine in the Gospel of St. John" by which "a clear move is made toward the modification of the unequivocal personal unity of the Semitic God" and together with this "the insistence on immanence". Dr. Whitehead reminds those who may have forgotten the fact in view of the Church's later gradual return to "the Semitic concept with the addition of the three-fold personality", that "immanence is implicit in various parts of the New Testament and was explicit in the first theological epoch of Christianity", which "was then Platonic—it followed John rather than Paul". (It is, however, at the least an open question to the most recent contemporary scholarship whether the Johannine Gospel, including its essentially pervasive Logos-doctrine, is not in its origins altogether a product of native Jewish religious soil. This mode of thought will then only later have become conscious of

its essential Platonic affinities. However, this does not alter the importance of the representation of immanence, though it may qualify its proportioning and have further significant implications.) It is relevant to quote here certain sentences from Dr. Whitehead's third, and most loftily metaphysical lecture. God, "who is the ground antecedent to transition, must include all possibilities of physical value conceptually, thereby holding the ideal forms apart in equal, conceptual realization of knowledge. Thus, as concepts, they are grasped together in the synthesis of omniscience." This seems indistinguishable from the historic doctrine of the archetypal presence of all the "ideas" in the "Word".

Remarking that "as a rebound from dogmatic intolerance the simplicity of religious truth has been a favorite axiom of liberalizing theologians", and commenting that "it is difficult to understand upon what evidence this notion is based" in view of "the complexity of interrelations disclosed in the physical world as science advances", our physicist continues that "in view of the horrors produced by religious bigotry, it is natural for sensitive thinkers to minimize religious dogmas. But such pragmatic reasons are dangerous guides. . . . All simplifications of religious dogma are shipwrecked upon the rock of the problem of evil". The main point is that "rational religion must have recourse to metaphysics for a scrutiny of its terms", while "at the same time it contributes its own independent evidence, which metaphysics must take account of in framing (its) most penetrating description of the universe". "You cannot shelter theology from science, or science from theology, neither can you shelter either of them from metaphysics, or metaphysics from either of them. There is no short cut to truth." "The foundations of dogma must be laid in a rational metaphysics which criticizes meanings, and endeavors to express the most general concepts adequate for the all-inclusive universe." Some golden paragraphs follow, upon the evasion of this (which has never been seriously doubted) arising out of the one-sided dominance of the historical interest in the last half of the nineteenth century, and the anti-metaphysical temper it engendered. " It is a curious delusion that the rock upon which our beliefs can be founded is an historical investigation. . . . Unless in this present you can find general principles which interpret the present as including a representation of the whole community of existents, you cannot move a step beyond your little patch of immediacy. Thus history presupposes a metaphysic. . . . This criticism applies equally to a science or to a religion which hopes to justify itself without any appeal to metaphysics. . . . But science can leave its metaphysics implicit and retire behind our belief in the pragmatic value of its general descriptions. If religion does that, it admits that its dogmas are merely pleasing ideas for the purpose of stimulating its emotions. Science, at least as a temporary methodological device, can rest upon a naïve faith. . . . When *religion* ceases to ask for penetration, for clarity, it is sinking back into its lower forms. The ages of faith are the ages of rationalism."

The author's earlier important work, "The Concept of Nature", began

with a citation of Plato's "Timaeus", and a notable feature of the searching analysis of the universe found in the two later works is its fundamental and striking affinity (adding the far greater elaboration and refinement of detail and the new horizons of relativity physics) with the—later so influential—account of the natural world anciently developed on the lips of Plato's Pythagorean. Unclear remain the ultimate relations of Dr. Whitehead's mutually irreducible "formative elements". "The present type of order in the world has arisen from an unimaginable past, and it will find its grave in an unimaginable future. There remain the inexhaustible realm of abstract forms, and creativity, with its shifting character, ever determined afresh by its own creatures, and God, upon whose wisdom all forms of order depend". And yet: "God is the one systematic, complete fact, which is the antecedent ground conditioning every creative act. The depths of his existence lie beyond the vulgarities of praise or power. He . . . transmutes what has been lost into a living fact within his own nature. . . . He is the actual fact from which the other formative elements cannot be torn apart. . . . The consciousness which is individual in us is universal in him; the love which is partial in us is all-embracing in him. . . . He is not the world, but the valuation of the world. . . . In the actual world he confronts what is actual in it with what is possible for it. Thus he solves all indeterminations." Why, under the implications of Dr. Whitehead's own thinking, should he be found to the end still declining technically to find in God "the ground of the whole metaphysical situation"?

If, though no moral fact can be changed, the ultimate religious insight is that *its value can*, the occurrence of evil at any point is not in principle any objection against the perfect goodness of the whole. If evil, *when overcome*, is justified, then, though it can never be held good that evil should be, or with absolutism that "eternally" it is good that evil should be, yet eternally it is good that evil should *have been*.

The further working out of his own so stimulating, profound and original thought on ultimate issues, by so great an *analytic* philosopher who has also been well characterized as "an almost perfect example of the *synoptic* mind" will be greeted with eager expectancy by the discerning.

EDWIN H. KELLOGG.

ENVIRONMENT AND RACE. By Griffith Taylor. Oxford, Oxford Univ. Press, 1927. Pp. xv+354.

Dr. Taylor, a Fellow of the Royal Geographical Society and head of the Department of Geography in the University of Sydney, is the author of several books on geography, meteorology, antarctica, and paleontology. "One of the main objects of this book", the author announces in the introductory chapter, "is to demonstrate that primitive man, being subject to the same laws as other animals, has evolved and migrated in response to similar stimuli". These stimuli are produced directly or indirectly by changes in topography and climate. "The fittest tribes evolve and survive

in the most stimulating regions . . . where living is not so hard as to stunt mental development, and not so easy as to encourage sloth and loss of initiative. The least fit are ultimately crowded out into the deserts, the tropical jungles, or the rugged mountains. . . . Only when conditions become uncomfortable owing to overcrowding or to a change for the worse in the environment, does the tribe or emigrant leave the familiar homelands. . . . The great migrations have been thrust out of Asia in four great waves during the million years of Post-Pliocene time. An attempt is made to correlate these thrusts with the four advances of the Great Ice Sheet from the Pole, which occurred within the same period. The explanation is admittedly far from complete, but the writer knows of no other theory which supplies so adequate a 'mechanism' to account for the migrations."

Part One presents the author's thesis (summarized in the previous paragraph) and a brief treatment of ethnological principles and criteria. The author suggests that "ethnologists would have done better to borrow from the geologist rather than from the botanist for an analogy to the ethnological processes": for a block diagram, such as the geologist uses, shows both the strata and the spread of races, whereas the conventional tree is inadequate for the depiction of both evolution and distribution; and it is a migration-zone theory of race evolution which the author advances. After disposing of nationality and language as inadequate racial criteria, Dr. Taylor proposes an "ethnograph" which includes five criteria, cephalic index, hair-type, stature, nasal index, and breadth of face. Both Taylor and Ripley (whom he quotes) believe the cephalic index the best single test of race.

Part Two, The Past, describes the topographical and climatic changes which have occurred in Australasia, Africa, Europe, Asia, and North and South America, and treats of the race distribution in these continents.

Part Three, The Present, gives an account of recent changes of environment and population in Australia, where the author has done the major portion of his research.

Part Four, The Future, deals with the potential white settlement of the world. Populations, Dr. Taylor states, depend on four chief factors, temperature, rain, coal supply, and altitude. Of the maximum white settlement of the future about one-half, he estimates, will live in North America, over one-third in the United States alone. Politically, two-thirds of the future white population of the world will be under either British or United States control.

Among the interesting controversial topics in the book, two will be of especial interest to psychologists at the present time, Keith's hormone theory, and the hypothesis of Nordic superiority. Granted that climate has produced changes in human nature, as Taylor and Huntington claim, the changes must be produced through some physiological agency. The endocrines are suggested as this agency. "Sir Arthur Keith points out that many of the criteria used by ethnologists have been definitely proved to be

affected by various glands in the body. Of these the chief are the pituitary and pineal within the cranium, the thyroid gland in the throat, and the adrenal glands near the kidneys. . . . Speaking generally, a rather moist, warm climate probably constituted man's early environment. The change to a moist, cooler climate in the Central Asiatic region may have stimulated one set of glands and hormones, and so brought about the changes in facial and bodily form which distinguish the Iberian from the negro. Further changes during the long ages spent by man in the same locality seem to have been in the direction of aridity; this may have affected other glands and hormones, and so developed the stocky, broad-faced, straight-haired Alpine and Mongolian peoples." However alluring this hypothesis, it must be remembered that our knowledge of the endocrines themselves is rudimentary indeed, to say nothing of the relation of the endocrines to so vital a matter as the evolution of racial "types".

The so-called Nordic races do not stand out as the most advanced type of man, according to Dr. Taylor. "The so-called 'yellow' or Mongolian type of man is a later product of human evolution . . . farther from the primitive anthropoid stock, while the negroid and West Indian peoples are earlier, lower offshoots from the line of human evolution." Most apostles of Nordic superiority use nationality as the essential racial criterion, whereas nearly every nation is a fusion of several racial stocks. "In the British Isles the people are possibly more largely Mediterranean than Nordic, but there is quite a strong mixture of Alpine blood." Such conclusions from the data of Army Alpha scores of foreign-born soldiers as Lothrop Stoddard and others draw seem altogether hasty and inaccurate. Race prejudice (which perhaps begets such conclusions, and certainly grows out of them) is, Dr. Taylor suggests, "but another name for ethnological ignorance". National jealousy "will tend to diminish as each nation realizes the place in the world's 'order of precedence' for which its racial, moral, and economic status equips it".

CHARLES LEONARD STONE.

MIND AND BODY. By Hans Driesch. Translated by Theodore Besterman. New York, The Dial Press, 1927. Pp. 191.

This translation, made from the third edition of *Leib und Seele*, with personal revision by Driesch, in 1927, is not very readable. The German text was bad enough, with its pedantic terminology and its overdivision into sections and subsections, which makes the exposition very jerky. There were needless repetitions, and, on the other hand, passages where the argument was so sketchy as to be scarcely intelligible to those not familiar with the author's earlier writings. Never was there anything limpid or flowing, any play of fancy, any grace of style. And now, added to the author's heaviness, we have a translation which, in its endeavor to be faithful to the original, gives us at times almost unintelligible English.

As for the argument, it may be pertinent to contemporary German discussion. But most of it has little relevance to the significant discussions of

recent years in English, which rest upon quite different analyses of the problem. It is a crying fault in philosophy that so much of it has a nationalistic horizon. In this little book there are one hundred footnote references, of which sixty-seven are to Driesch's own voluminous writings; in the other thirty-three references only four non-German writers are named. There is no sign of acquaintance with Bertrand Russell, Broad, or any of the other British realists who have studied the problem, no sign of acquaintance with Santayana, Strong, Morton Prince, or the many other American writers on the subject. It is a matter of plain fact that contemporary American discussions show a far wider mental horizon than this.

Nearly three-quarters of the book is taken up with a refutation of psychophysical parallelism. This theory is stated as the doctrine that there are "two separated realms of being and becoming", which are not causally related, but existentially parallel, at least in so far that every mental fact has always a "physical representative both in regard to its ultimate parts or elements and also in regard to the relations between such parts" (pp. 21–22). Moreover, the doctrine implies that "the Physical in its totality is throughout nothing but a mechanical system" (p. 23). "The Mental, of which parallelism speaks, is that which is *present in consciousness*, the Experienced as directly experienced, and it is to this that the Physical in the sense of mechanism is said to be parallel" (p. 35). The "issue", then, is "whether that which is consciously had, *as consciously had*, really *just as it is* can be parallelized with the Mechanical" (p. 109).

The doctrine being so stated, Driesch has a jolly time exhibiting its incredibility. But has he never heard of the "essence-doctrine", of the "sensum-theory", of "critical realism", of the "motor psychology"? Does he not know that there have been for some years parallelistic, or quasi-parallelistic, theories less naïve than the one he so gallantly attacks?

Chapter IV of Part I forms a sort of *enclave* in the argument, devoted to proving that "action is a non-mechanical natural phenomenon" (p. 64), and that, therefore, parallelism cannot be true, since it holds that the physical is nothing but a mechanical system. The arguments summarized in this chapter are Driesch's familiar arguments intended to show that the conduct of an organic body "contradicts most emphatically the concept of a machine" (p. 79). Organic bodies act as if they were "independent in their becoming", *i.e.*, autonomous. The famous "telegram-illustration" is repeated ("Fritz angekommen"—"Fritz-umgekommen"). . . . These arguments for "vitalism" have been patiently answer in detail, and space does not allow consideration of them here. It may merely be said that they are all arguments from ignorance: the vitalist does not see how such and such varieties of organic behavior can be conceived to take place mechanically. The way remains open for some cleverer or better informed person so to conceive them. . . . But this is all beside the point here. For there is no early reason why a parallelist need be a mechanist. If it seems likely, as investigation proceeds, that organic behavior is *not* completely describable in "mechanistic" terms, parallelists may be quite content. Doubtless most

parallelists have assumed the mechanistic theory of physical events, as the simplest and most probable hypothesis. But such an assumption has nothing whatever to do with parallelism itself, which can just as well adjust itself to a view of material events as partially capricious and unpredictable.

The earlier and later chapters of Part I undertake to show, by a faithful description of "mental things", that it is impossible to construct "mechanical equivalents" of them. Some of the statements made are, in themselves, questionable, as, *e.g.*, the assertion that "the ego-relationship is a part of all mental things, even of the most irreducible of our perceptual contents, such as the sensation of *red*" (p. 93). But by "mental things" Driesch means the data of consciousness, what is "present to the mind", what is "given". And it needs no laboring to prove the striking differences between them and the events which we may suppose to be going on in the brain. Such parallelists as suppose that the *data of consciousness* are "parallel" to brain-events are amply refuted, not only by Driesch, but by a number of contemporary parallelists, or panpsychists. The only "mental things" which may reasonably be supposed to be "parallel" to brain-events are mental *events*, not these essences, these meanings, which float before our minds.

All terms such as "ideas", "mental states", "thoughts", "perceptions", etc., are ambiguous. By "idea", for example, we may denote a mental event, an existing process, or we may denote an essence intended, imagined, meant. In the former sense, each "idea" has its own locus, is private, and nonrepeatable. In the latter sense, an "idea" is a fixed point for discourse; I may have the same idea over and over again, and you may have the same idea as I. . . . Driesch is talking all the time of "mental things" in the latter sense. For example, he talks of algebraic symbols, such as $\sqrt{2}$. *Of course* $\sqrt{2}$ is not the name of any existing mental event, which may be supposed to be "parallel", in some sense, to a brain-event. The mental event is *the process of thinking of* or *perceiving* that symbol. Such a mental event is complex, and of no fixed nature; it may contain visual, auditory, or lip-motor elements. What is important is that it shall have certain motor connections; what is fixed is its *function*, not its nature. To discover what any particular mental event is like is a job for introspection; and a difficult job it is, which at best can be very imperfectly performed, because our machinery for introspection is not very good. But to take mere logical entities, like $\sqrt{2}$, or the other objects of our contemplation which Driesch calls "mental things", and to try to make them "parallel" to brain-events is to talk nonsense.

Problems enough remain, when this distinction is made. What sort of mental events must exist, to account for the fact that such and such data are "given" to our contemplation? Can we believe that the qualitative differences which appear in our data exist in our mental states themselves? If so, how can what is qualitative be "parallel" to nonqualitative physical events? If not, how is it that qualities appear in our data? . . . These questions, and the many others which arise, were discussed in Strong's

The Origin of Consciousness, in 1918. The present reviewer's *Mind and Its Place in Nature*, published in 1925, formulates a theory to which none of Driesch's attacks are relevant.

Whether a theory based upon a correct analysis of the situation is to be called "parallelism" is a matter of arbitrary definition. Certainly such a theory is not likely to hold that there are two existent realms running parallel to each other. It may hold that physical knowledge and introspective knowledge give us two sorts of information, different but not incompatible, about one and the same event. Or that the term "mental" describes the *substance* of reality, while "physical" describes its *structural* aspect. Or some similar view which makes the parallelism apparent rather than a fundamental existential duality. But no allusion is to be found in this book to any but the most naïve form of the theory.

The final thirty-five pages of the text outline Driesch's own view. "My the same time the same something which enters nonmechanically into nature as a natural factor called 'entelechy' or 'psychoid' " (p. 161). This theory deserves careful study. But it has been presented in the author's earlier writings so much more fully and clearly than here, that we need not linger upon it in a review of this book, which is chiefly an attack upon parallelism.

It is to be noted that in these final, constructive pages Driesch makes the distinction between "processes in my mind" and "my direct conscious experience". Now why did he not bring this distinction to bear upon his discussion of parallelism? Doubtless because the parallelists whom he had in mind were German writers who are still carrying on the controversy in insufficiently analyzed categories. Perhaps this little book in German was a necessary counter-thrust at such opponents. But it does seem a pity that it was translated into English. For it will not enhance Driesch's reputation outside of that limited field of polemic. The impression upon well informed English and American readers will rather be of something provincial, almost archaic, and irrelevant to contemporary discussion.

Vassar College. DURANT DRAKE.

ABNORMAL PSYCHOLOGY. "WHEN THE OTHER-ONE ASTONISHES US." By Max F. Meyer. Columbia, Missouri, Lucas Brothers, 1927. Pp. viii+278; appendix.

This book may be regarded as a sequel to the *Psychology of the Other-One*. Here the author projects the principles advanced in that book into the realm of abnormal phenomena with interesting results. His six principles of human behavior are (1) the instincts and reflexes "as based on varying degrees of structural complexity in the human 'brain', or better, the human 'nervous system' "; (2) concertedness of action on part of the nervous system; (3) preoccupation "due to a reduction essentially temporary of the resistance of a contact . . . by increased nearness of two elements"; (4) deflection of one nerve current by another; (5) susceptibility of neurones; (6) generalization or "symbolic reactions of the muscle systems". These principles applied explain psychasthenia and neurasthenia

as due to an unusually low conductivity of all the habit centers; hypnotism and hysteria, to an unusual proneness to abnormal preoccupation; mania and depression, to "Sudden variation, up or down in degree of nervous conductivity of the nervous system". Mania is considered as "simply a lesser degree of the epileptic's abnormality and no more".

It is important to note that Max Meyer was among the first antagonists of a consciousness-psychology (to which he is wont to refer as the study of spooks, ghosts, and other mysterious entities) as evidenced by his articles twenty years ago and his *Fundamental Laws* (1911) where he made short work of introspection and of consciousness. The viewpoint of the author suffers from the sheer anecdotalism resorted to for illustrative material. Experimental literature in abnormal psychology is omitted entirely, clinical reports are few and far between and the only cases presented are taken from Janet, admirably reinterpreted. Why Lady Macbeth merits so much space is beyond us.

The author's criticism of "Psychanalysis" is a common ground between himself and Hollingworth. But the author does not go the way of the latter. For him *preoccupation* is a better term than *redintegration*, for redintegration is a subjective and metaphysical term. The tables are turned logically when the "mysterious" connotation of the no less but still more subjective term *preoccupation* is pointed out. His point that redintegration does not apply to all cases has already been taken into account in *Mental Growth and Decline* where redintegration is only one among several other criteria of functional disorders. The claim that neural levels are innumerable and hence *three* redintegrative-levels (autonomic, postural, and cortical, which are said to revive the ancient intellect-will doctrine) is an absurdity falls to the ground when it becomes evident that the one speaks in terms of neurology and the other in terms of neurology, a fallacy to be avoided as C. K. Ogden points out. (*Meaning of Psychology*, p. 26: double-language hypothesis). The assertion that redintegration reintroduces the Herbartian-conflict is unfounded.

One becomes curious to inquire for the "raison d'etre" of the dogmatic tone of the author in holding up his diagrams and principles as if constituting the iron-clad ultimates of human behavior and experience. Are there not many similar diagrams in Spencer (connections of nerve-fibers), James, and McDougall? Did not Thorndike present diagrams in 1905 which seem to be the same diagrams only not so thoroughly worked out? As for the author's law of double stimulation necessary for habit formation it sheds no new light for it is a fiction and requires too many assumptions altogether. The exact relation between the deflection of a nerve current as stated by the author and McDougall's drainage theory (which he in turn attributed to James' concept that "B drains A") should have been discussed by the author. Nor can the idea of stretching and sticking of dendrites be considered as a scientific conception; in 1911, Ladd and Woodworth indicated that "the balance of evidence is against these movements, but inner changes of a chemical and electrical nature might have

the same results''. As for the author's reiteration of the criticism of the ''common-path fallacy'' (Sherrington) is not the difference of opinion due to verbal differences? The author rejects as unthinkable the possibility of a nervous flux from a sense organ being dammed up in the brain, and points out that a ''brain stop'' and a ''brain start'' are inconceivable in terms of physical-physiological chemistry, that no psychologist who teaches ''dammed up energy'' has ever suggested a possible answer to the question ''what dammed?'' In a footnote he directs his criticism on Woodworth's textbook of psychology. In reply to this the reviewer has the following note from Professor Woodworth:

''This concept is not intended in the least to go outside of the domain of neuromechanics, or to imply the existence of some supra-neural 'dammer'. On the contrary, it is an attempt to envisage certain behavior facts in terms of neuromechanics. The behavior facts in question are those of delayed reaction, 'set', etc. It is perfectly clear that the effect of a stimulus is not instantly exhausted; the only question that can be raised is as to whether the hold-over is cerebral or involves some back-and-forth play between the center and the periphery. The ''damming'' is intra-organic, certainly; whether it is cerebral is not so certain. But there are some facts of reflex action, such as 'after-discharge', and the rhythmical series of movements sometimes obtained (as in the scratch reflex) from the action of a momentary stimulus, that indicate that the discharge of a nerve center is not always a mere instantaneous affair. Sometimes the nerve center, when aroused, persists in its activity for at least a brief period.''

There are many suggestive ideas scattered throughout the text. It is an interesting point the author makes about the function of undeveloped neurones, of which millions are still undeveloped at the time of death, in the shortening of the sensori-motorpath in learning. One wonders whether mental development cannot go on with increasing employment of these undeveloped neurones and whether they supply a neural basis for the reserve mental energies of William James and Boris Sidis. There is a very interesting paragraph on the control of dreams through the preoccupation. As for the general interpretation of dreams as after-tales of the dreamer witnessed by his listeners, it seems to disregard the fundamental ''other-oneness' psychology of the author, for is not an ''after-tale'' a distinct introspective datum?

NATHAN ISRAELI.

BRAINS OF RATS AND MEN. By C. J. Herrick. Chicago. Chicago Univ. Press, 1926. Pp. xiii+382.

Although much experimental work, both neurological and psychological, has been done on rats, and although five chapters in this book are devoted to such experimentation, the appropriateness of the title of the book may be questioned by some readers. The title was seemingly inspired by Burns' poem, ''To a Field Mouse'', the two final stanzas of which are quoted at the beginning of the concluding chapter. For those less poetic than Herrick

an alternative title might be "Cortex, Consciousness, and Civilization"; for the author states that "the human cerebral cortex is the specific organ of civilization", and that consciousness as a cortical function makes civilization possible.

The initial chapter, What We Do with Our Brains, introduces the major themes. The next seven chapters, technical in nature, deal with the function and evolution of the cortex and underlying brain-centers. Five chapters are devoted to experimentation on rats, and to neurological deductions from such experimentation. Chapter XIV skillfully arrays the facts of cortical evolution which have led to the author's point of view regarding the essential problems of consciousness. These problems constitute the substance of the five concluding chapters.

The inadequacy of the prevailing stimulus-response psychology, through its oversimplification, Herrick discloses much more effectively than either the Gestalt psychologists or the motivationists. The stimulus-response psychologists have not sufficiently realized the complexity of the nervous system: even in the simplest reflex there is some spread of excitation, and our simplest responses are the result of the interaction of a complexity of excitations. Similarly, we must recognize that "reaction is not merely transfer of energy applied at the receptor surface and released at the effector organ like the working of a lever. . . . A slight stimulus may . . . be followed by a torrential expenditure of energy quite out of proportion" to the intensity of the stimulus. There is need, too, for fuller neurological understanding of the conditioned reflex: if such a reflex is initiated and made effective through a principle of neural drainage or induction, the reflex is a much more complex one than is commonly assumed. "The simplified neurologic schemata which we use as pedagogic devices are necessary instruments for unlocking those doors of the student's mind which lead into the more recondite secrets of nervous organization. But having opened the door in this way, the key should be laid aside and not regarded as even an adequate symbol of the actual organization." Herrick does not question the mechanistic conception of the reflex; but he does question how far we can extend such a conception, whether it is an adequate basis for a true understanding of all human behavior.

Another line of neurological investigation and evidence which the strict stimulus-response psychologists neglect is that of cortical evolution. In reducing the behavior of rats to complexes of innate impulsions and crystallized habits, and then extending the principles thus established as explanatory rules of human behavior, the false assumption is implied that the brains of rats and those of men are alike. Herrick recognizes in evolution four major types of cortical structure: the ichthyopsid type (fishes and amphibians), with no well-differentiated cortex, though cortical primordia are evident; the sauropsid type (reptiles and birds), with cortex clearly defined, but limited in amount and of very primitive structure; the mammalian type (infra-human), with cortex expanded and more elaborate in internal texture; and the human type, in which the association centers are greatly enlarged and the texture highly complicated. A study of the behavior of

representatives of these four types of cortex shows that the increase in complexity of behavior is at least commensurate with the increase in neural complexity. It is, therefore, unscientific to apply to human behavior the principles adduced from the study of behavior of animals of lower neural types.

A third weakness of the strict stimulus-response psychology comes from its discarding consciousness and mental acts. Herrick thinks that the radical behaviorist's aversion to consciousness was generated by "the structural psychologist's figment of a detached or detachable psychic entity . . . which eludes scientific study". Consciousness to Herrick, however, involves nothing metaphysical: "the mental processes and their organs, like the muscular and conducting processes, have grown up in individual and racial development gradually and naturally" as correlates of neural structures which have grown up no more naturally. "No abyss of ignorance of what consciousness really is, no futilities of introspective analysis, no dialectic, destroy the simple datum that I have conscious experience and that this experience is a controlling factor in my behavior. . . . It is a travesty of the scientific method to leave out of consideration in a total view of human behavior just those characteristics which differentiate man from brutes and upon which the further progress of civilization must depend because we do not like to use the only satisfactory data now available for the study of these characteristics."

These matters, the complexity of neural action, cortical evolution, and consciousness as a datum for a biological psychology, are presented with a fascinating array of facts, a scholarly explanation and criticism, and a clear appreciation of their meaning for the future study of human behavior.

CHARLES LEONARD STONE.

THE MEANING OF PSYCHOLOGY. By C. K. Ogden. New York: Harper & Brothers, 1926. Pp. xxi+326.

Written as a survey of psychology for the general reader, this stimulating book is of the first rank as an original interpretation of the material presented. It is a criticism of contemporary work in the field from the standpoint of dynamic psychology (akin to that of Woodworth) in the light of recent advances in experimental research on chronaxy.

There is very little of chronaxy in the literature, although it seems to be a highly valuable concept for psychologists. In 1911, the originator of the theory, L. Lapique, suggested a theory of emotions based on it. The author applies chronaxy to many concepts: conditioned reflex (as memory) is a matter of an attuning of the brain centers; interests, attunement and acquirement of needs. *Attunement*—that is the significance of the chronaxy theory for Ogden. [The applications can be further extended. There is probably a fundamental relation between the logarithmic chronaxy curves of Lapique—*La Chronaxie*, 1926—and the curves of psychophysics. Chronaxic attunement offers suggestive clues to mental-set, reaction-time, attention, dynamogenesis, aesthetics. Music might be conceived of as tuning up the mind. Prepotency—Sherrington, Allport—would be a prechronaxic attunement.]

There is also a well developed theory of interests. An interest is an activity set going and maintained by a need. It is a matter of excitability and attunement of the centers concerned. Clear thinking and clarity of interests are interrelated. In the abnormal realm extraordinary views and attitudes and "eccentric interests" are interdependent.

Chronaxy and interests are the author's two favorite criteria for studying the schools in psychology. Freudianism is really bereft of its animistic taint when the unconscious, repression, and regression are tied up with the criteria mentioned. Thus, in regard to wishes they are said to have no underground persistence in the mind "any more than the return of spring each year is a proof that she has been lurking in the soil throughout the winter. What persists in all cases is a certain pattern in the organization of the mind". Conditioning is not only an attunement but it is subject to specific, dominant interests. "In actual fact we pick out the pattern which best fits in with the dominant activity. Pavlov's dogs pick out the note and treat it as a signal for salivation because food-seeking is their dominant activity." Now, a "*Gestalt* . . . is a disposition, conceived not as a fixed pathway among neurones, but as a system which has a certain end-state, a certain poise to which it tends under a given set of conditions". The author commends the insistence of behaviorism on stimulus response and condemns its ruling out of consciousness as "merely waste of time". In a footnote he includes as an instance of the logical muddles which haunt behaviorists Roback's witticism on Pillsbury's story of the wide awake individual who dreamed. . . . He finds a close relationship between the "boomerang" analogy of Watson and the Gestalt viewpoint on instinct, between the conditioning of the love emotion and views of psychoanalysts like Kempf and Frinck.

Indeed, verbal differences account for divergent psychologies and language, though an instrument of economy and efficiency in adaptations to environment, has many drawbacks. Language early conditions the growing child to a traditional outlook upon intellectual and other matters. Ogden criticizes general opinion and thought as for the most part nothing "but language operated at a merely emotional level". To emphasize the necessity of control of words, he points out that a "truly sagacious Dictator would make it his first business to create a Word-conscious Proletariat".

The author is fascinated by the deep seas of his subject. He finds psychology at the bottom of physics. The solution of the riddle of the universe lies with psychology.

An ultimate interpretation of nature rests upon the reliability of the mind as an accurate photographic mechanism for viewing the cosmos. The final epistemological critique is psychological and therefore the *mind* must understand the make-up and functions of the mind.

The author's outlook is a *futuristic* one. He interprets recent progress in science as amounting to "getting the guns into position". Relativity of time means for him that we "might under certain circumstances know the past from the future instead of the future from the past"!

Columbia University. NATHAN ISRAELI.

NEW BOOKS RECEIVED

(This notice does not preclude the book from further notice or review)

The Psychology of Individual Differences. By Robert S. Ellis. Appleton and Co., New York, 1928. Pp. xxiii+533. Price $3.50.

Judgment and Reasoning in the Child. By Jean Piaget. Harcourt, Brace and Co., New York, 1928. Pp. viii+260.

The Psychology of Personality. By English Bagby. Henry Holt and Co., New York, 1928. Pp. viii+236. Price $2.50.

How to Do Research in Education. By Carter V. Good. Warwick and York, Baltimore, 1928. Pp. 298.

Human Behavior. By Walter S. Hunter. University of Chicago Press, Chicago, 1928. Pp. x+355. Price $2.00.

Psychology. By H. L. Hollingworth. D. Appleton and Co., New York, 1928. Pp. xviii+539. Price $3.00.

Mongolism. By Kate Brousseau and H. G. Brainerd. Williams and Wilkins Co., Baltimore, 1928. Pp. viii+210. Price $4.50.

Quantitative Methods in Politics. By Stuart A. Rice. Alfred A. Knopf, New York, 1928. Pp. xxii+331.

Plato's Theory of Ethics. By R. C. Lodge. Harcourt, Brace and Co., New York, 1928. Pp. xiv+558. Price $7.00.

The Lure of Superiority. By Wayland F. Vaughan. Henry Holt and Co., New York, 1928. Pp. viii+307. Price $3.00.

The Social Insects. By William M. Wheeler. Harcourt, Brace and Co., New York, 1928. Pp. xviii+378. Price $7.00.

Man and Civilization. By John Storck. Harcourt, Brace and Co., New York, 1928. Pp. v+449.

Psychology as Science. By H. D. Weld. Henry Holt and Co., New York, 1928. Pp. xi+297. Price $2.50.

The Matrix of the Mind. By Frederic Wood Jones and Stanley D. Porteus. University of Hawaii Press, Honolulu, 1928. Pp. 457.

Introduction Biologique a L'Etude Neurologie et de la Psychopathologie. By C. v. Monakow and R. Mourgue. Felix Alcan, Paris, 1927. Pp. xi+392.

Introduction a la Psychologie Collective. By Ch. Blondel. Colin, Paris, 1928. Pp. 206.

How Animals Find Their Way About. By Etienne Rabaud. Harcourt, Brace and Co., New York, 1928. Pp. viii+142.

An Introduction to Psychology. By Susan S. Brierley. Methuen and Co., London, 1928. Pp. vii+160. Price 5s.

Psychology for Executives. By Elliott Dunlap Smith. Harper and Brothers, New York, 1928. Pp. xii+262. Price $3.50.

The Pragmatic Revolt in Politics. By W. Y. Elliott. Macmillan, New York, 1928. Pp. xiii+540.

Fundamentals of Objective Psychology. By John Frederick Dashiell. Houghton Mifflin Co., New York, 1928. Pp. xviii+588. Price $3.00.

The Psychology of Learning. By William Henry Pyle. Warwick and York, Baltimore, 1928. Pp. 441. Price $2.32.

The American Negro. By Melville J. Herkovits. Alfred A. Knopf, New York, 1928. Pp. xiv+92.

A New God for America. By Herbert Parrish. The Century Co., New York, 1928. Pp. xxvii+268.

Rainbow Round My Shoulder. By Howard W. Odum. The Bobbs-Merrill Co., Indianapolis, 1928. Pp. 323.

Coming of Age in Samoa. By Margaret Mead. William Morrow and Co., New York, 1928. Pp. xv+297. Price $3.00.

Seven Days Whipping. By John Biggs, Jr. Charles Scribner's Sons, New York, 1928. Pp. 219. Price $2.00.

Living With Our Children. By Lillian M. Gilbreth. W. W. Norton and Co., New York, 1928. Pp. xii+309. Price $2.50.

Problems of College Education. University of Minnesota, Minneapolis, 1928. Pp. ix+449. Price $3.00.

Infancy and Growth. By Arnold Gesell. Macmillan Co., New York, 1928. Pp. xvii+418.

The Fundamentals of Human Motivation. By Leonard T. Troland. D. van Nostrand and Co., New York, 1928.

AN ANNOUNCEMENT REGARDING THE NEW JOURNAL OF ABSTRACTS IN THE SOCIAL SCIENCES

The Social Science Research Council announces plans to establish a *Journal of Abstracts in the Social Sciences*. These plans are the result of five years study by a committee of the Council which has canvassed the situation with respect to the needs, resources, and purposes to be served by a comprehensive abstract service in the social sciences.

A substantial subsidy has been provided for a period of ten years until the journal has become self-supporting through subscriptions.

In its report to the Council at Hanover, New Hampshire in August, 1927, the Committee on Social Science Abstracts stated the need for abstracts in the following paragraphs:

"The founding of the Social Science Research Council is itself a recognition of the fact that leaders in the social sciences are convinced that research in these disciplines is greatly in need of stimulation and direction, and farther, that the scholars in these fields should be brought closer together for the consideration and solution of common problems. On the other hand, the deliberations of the Committee on Social Science Abstracts, and much of the information gathered by it, clearly bring out the fact that one great obstacle to the doing of truly scientific research in these fields lies in the tremendous mass of the materials to be considered and in the relative, if not quite complete, lack of appropriate tools for attacking it. There are so many books, pamphlets and reports constantly being published and so many periodicals both scientific and semi-scientific steadily pouring from printing houses both here and abroad, that it is physically impossible for any one to keep abreast of all the literature even in his own special field of work. For this reason also, and in making a courageous effort to read what he should, he is likely to take first the publications obviously in his own special field, and for lack of time to do more, to become increasingly oblivious of what is being done in other disciplines upon the same subjects. Thus artificial departmental lines tend to become sharper, and in his mind the social sciences stand as distinct and separate fields.

"To overcome these difficulties, a journal is proposed which will save an almost infinite amount of time and labor on the part of research workers, by giving them in one journal complete citations and short but objective abstracts of all important new materials, and will at the same time draw together the several disciplines by serving them all through one journal based upon some systematic classification and improved by numerous cross-references to the materials in other fields. Other important advantages of such a publication could easily be stated. It will save much duplication and waste of effort, it will apprise the worker of the existence of other specialists working on his problems and stimulate correspondence between them, it will call attention to new methods of research, it will serve as a permanent record of the work already accomplished, and will in many other ways promote the healthy development of the sciences to which it relates."

The Social Science Research Council has appointed an Organizing Committee consisting of the following scholars, and charged with the responsibility of organizing and establishing *Social Science Abstracts:* Dr. Isaiah Bowman, American Geographical Society; Dr. Davis R. Dewey, Editor of the American Economic Review; Dr. Carlton Hayes, Professor of History, Columbia University; Dr. Frederic A. Ogg, Editor of the American Political Science Review; Dr. Frank A. Ross, Editor of the Journal of the American Statistical Association; Dr. Clark Wissler, Professor of Anthropology, Yale University; and Dr. F. Stuart Chapin, chairman, Professor of Sociology, University of Minnesota.

To assist the Organizing Committee, a number of advisory committees have been appointed in the fields of cultural anthropology, economics, history, human geography, political science, sociology, and statistics. These advisory committees have been asked to suggest: (1) the names of scholars who may be considered for the position of salaried editors and unsalaried consulting editors; (2) to draw up a scheme of classification adequate to the needs of the systematic grouping of materials from their respective fields of specialization within the social sciences.

Since the Council is made up of delegates from the national learned societies in the fields of anthropology, economics, history, political science, psychology, sociology, and statistics, the purposes of the Council in its efforts to further coöperative scientific research in the social sciences is best served by devoting *Social Science Abstracts* to the fields of cultural anthropology, history, human geography, political science, sociology, and statistics, broadly construed.

Social Science Abstracts will be issued monthly during the year, and in each issue will appear systematic abstracts of new information published in the fields indicated for the preceding month or months. *Social Science Abstracts* will be printed in English in this country, but it will attempt to cover the social science literature of the world as originally published in all languages.

Negotiations are under way to establish a satisfactory basis of coöperation with the Committee on Intellectual Coöperation of the League of Nations, in working out a modus operandi with the arrangements for economic abstracts undertaken by this international organization.

The test of published materials to be abstracted will in general be the criterion of *new information*, in the sense of important factural studies and contributions to theory and opinion, in the fields of the social sciences indicated. This will require the careful scrutiny of articles in periodicals, pamphlets, bulletins, monographs, and new books. It is conservatively estimated that the annual number of abstracts will run to fifteen or twenty thousand titles the first year. The abstracts will be cross-referenced and annual indexes published. It is hoped that the first number of *Social Science Abstracts* may be published within the present calendar year.

F. Stuart Chapin,
Chairman of the Organizing Committee.

Directory of American Psychological Periodica

American Journal of Psychology — Ithaca, N. Y.: Morrill Hall. Subscription $6.50. 600 pages annually. Edited by E. B. Titchener. Quarterly. General and experimental psychology. Founded 1887.

The Pedagogical Seminary and Journal of Genetic Psychology — Worcester, Mass.: Clark University Press. Subscription $5. 520 pages annually. Edited by Carl Murchison and an international coöperating board. Quarterly. Child behavior, differential and genetic psychology. Founded 1891.

Psychological Review — Princeton, N. J.: Psychological Review Company. Subscription $5.50. 480 pages annually. Bi-monthly. General. Founded 1894. Edited by Howard C. Warren.

Psychological Bulletin — Princeton, N. J.: Psychological Review Company. Subscription $6. 720 pages annually. Psychological literature. Monthly. Founded 1904. Edited by Samuel W. Fernberger.

Psychological Monographs — Princeton, N. J.: Psychological Review Company. Subscription $6 per volume. 500 pages. Founded 1895. Edited by Raymond Dodge. Published without fixed dates, each issue one or more researches.

Psychological Index — Princeton, N. J.: Psychological Review Company. Subscription $4. 300–400 pages. Founded 1895. Edited by W. S. Hunter. An annual bibliography of psychological literature.

Journal of Philosophy — New York; Sub-Station 84. Subscription $4. 728 pages per volume. Founded 1904. Bi-weekly. Edited by F. J. E. Woodbridge and Wendell T. Bush.

Archives of Psychology — Sub-Station 84, N. Y.: Archives of Psychology. Subscription $5. 500 pp. ann. Founded 1906. Ed. by R. S. Woodworth. Published without fixed dates, each number a single experimental study.

Journal of Abnormal and Social Psychology— Boyd Printing Co., Inc., Albany, N. Y. Subscription $5. Edited by Morton Prince and Henry T. Moore. Quarterly. 432 pages annually. Founded 1906. Abnormal and social.

Psychological Clinic — Philadelphia: Psychological Clinic Press. Subscription $2.50. 288 pages. Ed. by Lightner Witmer. Founded 1907. Without fixed dates (9 numb... Orthogenics, psychology, hygiene.

Training School Bulletin — Vineland, N. ... The Training School. Subscription $1. ... pp. ann. Ed. by E. R. Johnstone. Foun... 1904. Monthly (10 numbers). Psycho... and training of defectives.

Comparative Psychology Monograph... Baltimore: Williams & Wilkins Co. S... scription $5. 500 pages per volume. Ed... by W. S. Hunter. Published without f... dates, each number a single research.

Psychoanalytic Review — Washington, D. ... 3617 10th St., N. W. Subscription $6. ... pages annually. Psychoanalysis. Quarte... Founded 1913. Ed. by W. A. White ... S. E. Jelliffe.

Journal of Experimental Psychology — Pri... ton, N. J.: Psychological Review Comp... 500 pages annually. Experimental. S... scription $6. Founded 1916. Bi-month... Edited by Madison Bentley.

Journal of Applied Psychology — Worce... Mass. Florence Chandler. Subscription ... 400 pages annually. Founded 1917. Q... terly. Edited by James P. Porter ... William F. Book.

Journal of Comparative Psychology — Ba... more: Williams & Wilkins Co. Subscri... $5. 500 pages annually. Founded 1... Bi-monthly. Edited by Knight Dunlap ... Robert M. Yerkes. (P. ...

Genetic Psychology Monographs — Worce... Mass.: Clark University Press. Subsc... tion $7.00 per volume. Two volumes ... year, 600 pages each. Edited by ... Murchison and an international coöpera... board. Monthly. Each number one ... plete research. Child behavior, differe... and genetic psychology. Founded 192...

Journal of General Psychology — Worce... Mass.: Clark University Press. Subsc... tion $7.00. 600–700 pages annually. Ed... by Carl Murchison and an international ... operating board. Quarterly. Experime... theoretical, clinical, and historical ... chology. Founded 1927.

Psychological Abstracts — Princeton, N. ... Subscription $5.00. 700 pages ann. Ed... by W. S. Hunter. Monthly. Abstract... psychological literature. Founded 192...